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FORM AND MATTER, I

SOME such contrast as that of form and matter would be common intellectual property, no doubt, even if Aristotle and Kant had not made such relentless metaphysics of it. As things are, the Aristotelian notion in particular has probably affected more human beings than any other philosophic concept. Few doctrines are easier to explain in a textbook paragraph; almost none is harder to understand in detail and principle. This does not disturb the traditionalist for whom its antiquity and versatility *in usum ecclesiae* put it above serious criticism, nor the modernist for whom its association with exploded science has made it beneath contempt. It is a trial, however, to two ingenuous sorts of citizen, the classical scholar who wishes to decipher what Aristotle was driving at, and the student of ontology who is looking for principles to explain things by. I have no pretensions to classical scholarship, so it was a certain involvement of the latter sort which set me plodding in some of the Peripatetic tracks. I am accordingly grateful for opportunity to put the issue before better informed persons, hoping that if I can interest them with an outsider's canvass of some things which Aristotle may have meant, they will help toward some verdict on the truth of the business.[1]

The trouble seems to have lain at the roots of the doctrine, growing out of Greek common sense and older philosophies where it was vague and legendary except for the materialistic version which Aristotle could not abide. This native obscurity the Aristotelian school cultivated like a fine flower by means of the famous method of analogy of proportionality. Form, we are told, is to matter as the shape of the statue is to its bronze, as the educated man to the uneducated, as health is to gall and phlegm, and as specific difference is to genus, and so on for a repertory

[1] This was written for a meeting of the Society for Ancient Greek Philosophy at Harvard University on December 27, 1957. Though not unaffected by certain clues and countersuggestions kindly offered there, I think they do not yet demand any substantial revision of this inventory.

of illustrations repeated with dreamy obstinacy for centuries. This leaves us, of course, at the mercy of the Philosopher's thumping errors of fact; but more seriously it is delusive in principle, because any pair of things "are to" one another, that is, are related, in a virtual infinity of ways, and in any finite set of such pairs countless of these relations will resemble and countless will differ. An arithmetical proportion is perfectly intelligible, though unexciting, because the abstractions are already familiar and we understand just what relations are being compared. The philosophical method of analogy, however, starts at the other end, expecting to identify the intended relation or its grounds from only the statement or hunch that it is one among those shared in a set of pairs; and it lacks just what makes the predigested arithmetical proportion reliable. In abeyance of the abstraction which the method is used to avoid, only with good luck could Aristotle have identified the same respect of resemblance among all his pairs of pairs to begin with, and only with more luck could we identify the same respect after him.

One reason why the mystery has nevertheless seldom silenced us in the classroom is, of course, the conventional pretense that anyone who took the trouble to look the subject up would find it all worked out somewhere. But there is a better reason in our easy access to at least one version of the distinction which is not analogical but abstract and clear: form is structure, and matter is what has or enters into the structure; and the structure of anything is a relation or a set of relations or a sum of relations among its parts. Each part or term is designatable as "matter" for the form, "content" for the structure; but "the" matter or content for it is strictly the polyad of terms, or set of parts, subject to the relation, and less strictly is their sum, the concrete thing minus the stipulated relations. The same complex of relations thus relates the parts, is the form for which they are matter, and is the form *of* the total thing, parts and relations together.

The principle of structure, I think, is indeed the one substantive element in the Aristotelian notion, and is what gives professional dignity and endurance to it. The standard pedagogic epitome of hylomorphism is that "the matter is the stuff a thing is made of, the form is the way it is put together." And prominent in the

secular scholar's offhand description of Aristotelian form is "intelligible structure."[2] Greek culture seems to have been fascinated with order and rhythm of arrangement and resemblance—aesthetically, mathematically, and then metaphysically. The "forms" advocated by the Pythagoreans and Democritus were configurations with countable junctures, and Plato after them continually thought of his Forms as like a carpenter's plan which the world's lumber is arranged to match. Of Aristotle's words for form, the majority—*εἶδος*, *ἰδέα*, *γένος* (194b 26), *παράδειγμα*, *λόγος* (996b 7), *φύσις* (193b 17), and even *μορφή*—might by then have meant almost anything, but most of them were redolent of the structuralistic tradition, I should suppose, and his *σχῆμα* must have reminded his colleagues of his own "figures" of the syllogism and of Democritus' use of the word for essential principles which had to be structures of the plainest spatiotemporal sort, while the occasional *τάξις* is confirmatory evidence. His first official example of a formal cause is the ratio 2:1 (194b 27), and a little later we hear that "all excellences depend upon particular relations" (246b 4). His favorite illustrations are "arrangements" or "structures" in the literal sense that they have been arranged or constructed, either by themselves, as living things are, or by art, like the statue, the rudder, the house. The shape of the statue, I need hardly point out, consists of the distances and directions among its parts; and though Aristotle's science of mixed and blended qualities is more elusive, if we once can credit such metaphysical pharmacy we must grant that the results would be "structures" in the broad and proper sense. The great avatar, Kant, finally, who plumed himself that he was using the Aristotelian distinction, made no bones about it that "form" is relational structure.

[2] Thus Professor Morrow in the Runes *Dictionary of Philosophy* (New York, 1942), art. "Form"; W. D. Ross, *Aristotle* (London, 1930), p. 74; etc. Ostensible quotations from Aristotle hereinafter are mostly from the Ross translations. My documentation is a little sporadic because most of the allusions are commonplaces, to ideas much repeated in Aristotle's works, or familiar resultants of many dispersed remarks. The references are meant mainly to identify the phases of Aristotle's argument which are intended, mainly in the *Physics* (184-267) and *Metaphysics* (980-1093), rather than to be proofs of my interpretation.

Many of the very discrepancies and vacillations in the Aristotelian corpus only confirm the structuralistic interpretation, for they are just what structuralism would make natural. Thus Aristotle writes of "the" form of a thing, as if it had just the one form, whereas anything demonstrably has a virtual infinity of structures—every set of a thing's parts providing as many structures as there are relations among them, and there being a stupendous if not infinite number of sets of parts. But this is the sort of wholesome lapse made by people who unquestionably are thinking about structure and would explain that by "the structure" they mean some special or salient structure, much as Aristotle concedes that in strictness a thing has many forms if we count nonessential ones. Our interpretation explains, again, Aristotle's mild wonder that the form not merely in some degree depends causally on the matter but sometimes *includes* the matter (1033a 4)—a saw by definition must consist of iron (200b 6), though a man, I gather, need not consist of meat (1036b 5). For it is customary, and doubtless indispensable, to count qualities of terms as constitutive of structures and even of relations, as the specific colors of parts of a flag, no less than their locations and contrasts, are essential to its pattern. Then again we explain Aristotle's quandary about whether the form of a thing is, as he often says, a part or constituent of it (1033b 19, e.g.), or, as he says at least once, not a part but a "principle" of unity (1041b 30). For what we confront here is the primordial and subtle difficulty for any analytic philosophy, which Russell was to wrestle with again in his *Principles of Mathematics*,[3] that while the relations among the parts of a thing are in a sense constituents too, and though we can list the relations with the terms, the form with the matter, we thus leave out, that is, fail to convey, what can only be expressed not by a list but by a proposition: how the relations are operative, that they relate these terms in this order, that the form is of this matter. Aristotle's apparent belief (983b, etc.), approved by Ross as well as by more religious champions,[4] that for this

[3] Bertrand Russell, *Principles of Mathematics* (New York, 1943), p. 141.

[4] Ross, *Aristotle*, p. 172; introduction to his edition of the *Metaphysics* (Oxford, 1953), I, p. cxiii; Jacques Maritain, *Introduction to Philosophy* (New York, 1930), p. 167. Aristotle in the passages I cite is complaining that the pre-Socratics

reason form is an "immaterial" principle in a sense hostile to philosophic materialism is a mistake, since the problem of relationality is as integral to physical structures as to spiritual ones, but it too is the kind of error to be expected if "form" means relational structure.

There is a profounder deterrent to the structuralistic interpretation of form in that the Aristotelian, who belittles relations as "least of all a kind of entity" (1088a 22), and at a showdown denies that substances can so much as contain actual parts to be related (1002a 20, 1040b 5), cannot with good grace admit relational pattern into the very core and essence of things. But this barrier to our hypothesis must go down anyhow, not just because the Aristotelian canon of relations demonstrably requires general revision, but because the Aristotelian relaxes it here himself. The irony is that though Aristotle and his Scholastics seldom or never say that a form is a structure consisting of relations among a thing's parts, they say not only that the form and matter are parts but that they are related parts. Aristotle does this tacitly when he introduces form and matter by analogy, for he defines analogy as a resemblance of relations. He does it expressly when he calls form and matter "relative" because of how they causally depend on each other (194b 9). Neo-Thomists labor it most when they insist that form and matter can be understood only by their "transcendental relation" and "the hylomorphic structure" this composes.[5] If the μορφή now is a structure too, the hylomorphic structure is the relation between relations and their terms, and it deserves the title "transcendental" if only because *its* terms belong to different categories or types.

That a form consists of relations, and that it is related to its matter, are two senses in which the form-matter distinction may be said to be "relative," but most famous is a third way, that what is form relatively to one batch of being may be matter relatively to another, and this on the whole our structuralism also accommodates, finding in it again only about the degree and kind of error one would expect—to quote Aristotle's way of condescending

were obsessed with "the material cause"; but in fact, of course, they wanted to explain everything by differences of *structure*.

[5] See Louis de Raeymaeker, *Philosophie de l'être* (Louvain, 1947), pp. 191, etc.

to his predecessors (993a 16, etc.)—in the lisping infancy of philosophy. For Aristotle is right enough that structurally a man is to his organs as each organ is to its tissues, and as these are to the cosmic elements, namely, in that he consists of them in a certain combination. We need only guard against saying that *this* relation is that of form to matter; it is only that of whole to part, a structural relation at one level being not a term for but a constituent of a term for a structural relation at the next "higher" level. Only when the inclusive relations relate the included relations themselves is it strictly true that the form at one level is matter for the next level, and this is a refinement which Aristotle, who found it hard enough to instruct the Athenians about plain everyday relations of things, can be excused from elucidating.

Very quickly now, however, the hierarchical theme which haunts all Aristotelianism joins with others of its dominant motives to carry us to where the structural interpretation fits so badly that either the philosophy is sadly in error about the nature of structure or it does not intend to equate form with structure after all. In the latter event it is a nice and hard duty to guess what it does intend. The departures are in two main directions, one which makes form in a clear-cut technical way a more general concept than structure, another which makes it, in a more obscure, elevating, and influential way, a more special concept.

The technical departure is most manifest, perhaps, at the supposed top and bottom of the form-matter hierarchy. A modern analyst, if he did not think the pattern of part and whole ran to infinity up and down, would hold that the top of the hierarchy is the structure of the world whole, into which everything enters as matter and which enters as matter into nothing, and that the bottom is supplied by terms into which no structure enters but which together enter into all structures. Being devoid of relational character, the latter must have or be simple qualia. Our analyst might sympathize with Aristotle's quest for primitive stuffs and qualities to provide content for things, and certainly would join him in reprehending Plato's disembodied Forms, which threaten the absurdity, not just of qualities without instances, but of relations without terms, a wilder notion than lefts

without rights. But what Aristotle in fact does, of course, is so gratuitously different from this that centuries of custom have not lessened the shock of it on critical minds. In defiance of his own tireless complaints against Plato, he affirms a top form which, so far from having everything for its matter, has no matter, but is pure form, and at the bottom end, where we might now expect, to match his form without matter, a matter without form, that is, consisting of terms which *have* no relations, we find instead neither this which his new thesis requires, nor the terms merely containing no relations which strict structuralism requires, but a stuff devoid both of structures and of qualities. This alone is prime matter, everything else, including the structureless qualia, being "form." I don't lodge the usual objection, which I think is false, that such a substratum is inconceivable, nor even object, what I think is true, that it provides a clumsy and incredible theory of predication. The fault is that the scheme has confounded together two quite distinct principles, the more special notion of relational composition, and the more general notion of predication or attribution. Though we grant that a man's body "is to" his organs as an organ is to its tissues and as the tissues are to constituent simple qualia, now when we have got down to the qualia we are at the end of that hierarchy or proportionality; by hypothesis there cannot be anything to which the qualia have the "ratio" the tissue has to them. That Aristotle has skipped to a fundamentally different principle is witnessed by how many modern analysts would think it imperative that relations should have terms but preposterous that qualities must inhere in a substratum. If Aristotle wants to say the latter too, as is his right, he must suppose that qualitative predication is itself an elementary structure, to yield still a different hierarchy, where the last structure is the union of quale and substratum, which are thus irreducibly and coequally prime matters.

Losing his way in the twilight of the analogical hierarchy was perhaps a main occasion for Aristotle's extending the title of "form" from structures to all characters or predicates, but it surely was not the only one. For though he often plays fast and loose with whether form or matter is preeminently a "this" (1042a 29, 1049a 26, 1070a 11), when he seriously distinguishes "this" from "such," the matter is the "this" (190b 25, 999a 33),

the particular subject, and the form is the "such" (1033b 22, 1038b 35), the characterizing attribute, just any such entity as "determines" a subject, in whatever category—structure, quale, or even the subject's relation to something outside itself (1042b 25, 1043b 7). To be a form is to be abstract and universal: to be abstract in that it cannot exist separately, "in itself," but only "in another", and to be a universal either actually and directly, identically present in each of many instances, as the school books generally say Aristotle held, and as I think was his conspicuous intention, or indirectly, by the operationalist idea fathered on him by the Thomists, that the form is anyhow the element in the thing which is fit to become a universal when the mind "abstracts" it. These specifications confirm that a quale is as truly a form as a structure is, since warmth, for example, is as incapable of existing without a subject, and as capable of existing in many subjects, as humanity or triangularity.

Aristotle's use of "form" in senses more hospitable than the notion of structure would permit does not, however, hinder him from a use which confines it to senses more special and honorific than structure requires and indeed than it can well support. In the new senses to be a "form" is a prerogative capable of degrees, some forms are more formish than others, and *the* form, the essential form, of a thing is its most formish form. And whereas the extension of the idea of form from structure to all characters seems only an incident in Aristotle's exposition, its specialization to apply properly only to superlative and prerogative forms is altogether central and entrancing. It is, however, correspondingly obscure, and I propose to follow now only the one clue, given by Aristotle's pretty plain relegation of qualia to the foot of the analogical ladder, that what is "form" in the special sense must be at least a structure, and to ask then whether the prerogatives assigned to it can be defined by traits of structure or even consistently grafted on them. In this connection we might have hoped to get light on prerogative "form" from the cognate notion of prerogative "essence" (οὐσία; τὸ τί ἦν εἶναι) which Aristotle often equates with it (1032b 1, 1044a 36, etc.), but I am convinced that a course proceeding in the opposite direction has anyhow the advantage of *obscurius per obscurum.*

There is nothing in the fundamental conception of structure to dictate what might count as more structure. Typically, I suppose, we would mean by "more structure" a structure which pleases or entertains us. We call a thing "deformed" or "amorphous" if we don't like or don't notice the shape, and "shapely" if we do, much as we describe a person or a thing as "quality" if it is of good quality and "classy" if of a desirable class. But even if we conjecture that professorial lickerishness first decided Aristotle on his verdict, quite ungrounded otherwise, that the form of wine is superior to the form of vinegar (1044b 33), still he must have been trying to apply a general principle of a more serious kind. The most obvious of objective associations of "higher form" would be with two abstractly logical sorts of "superiority," each a sort of inclusiveness, which loomed large to him already. One is taxonomic, by which the "higher" form, whether quale or structure, would be the more general one, of the greatest extension. The old philosophies, never very sound on extension and intension, were often victimized by this, but it can't be the main Aristotelian reliance, for it makes the most abstract and indeterminate character the "highest," and Aristotle is very repetitive that the genus is not more form than but is matter for the species (1024b 7, 1038a 6, etc.). The other logical criterion of inclusiveness is the strictly hylomorphic one, constituted not by how many instances a universal has, but by the scope of a particular relationship—the criterion by which in a hierarchy of nested matters and forms, the forms of wholes are "higher" than the forms of their parts. This has at least perspicuity and decorum, explaining why qualia gravitate to the bottom, and making the biggest things the loftiest ones. But "higher" here too is only a logical trope which, no matter what Aristotle may sometimes have thought, entails none of the Aristotelian prerogatives. The organization of a human cell may in some significant way be "higher" than that of a molecule in it, and the organization of a man "higher" than of the cell, but the state's form, in spite of Aristotle's occasional anticipation of the Hegelian view, is not superior to that of the individual, and the human race is pandemonium. That nothing important could have followed from rank in the hierarchy is evident when we remember that there can be a

square figure whose elements are circles, or a circle composed of squares, or a circle of circles, or a square of squares. (The degrees of form, therefore, since they coincide with nothing else we have found so far, call for still another explication of the maxim that the idea of form is "relative," that is, that it is comparative.)

There was another distinction bound up in the hierarchy which opens a more promising avenue: not the distinction between the structure of a whole and the structures of parts of the whole, but one between a whole structure and any part of *the structure*. For here the inclusive structure is indeed more structure than the included structure in the most primitive and unmistakable way, that it contains everything the latter does and more besides. Thus the salient organization of a man's neuromuscular system amounts to more than that of just his nervous system, even though they do not occupy any greater volume, because it contains more relations. And though it is rash to expand this principle to structures of which neither is a proper part of the other, we habitually do so, with what seems considerable reliability.

If we identify amount of structure with number of constituent relations—the number of dyadic relations, let us say, to calibrate the affair—we are identifying degree of structure with its "complexity" in the most naive sense of that word. Since on this plan we can always pass from less structure to more by so simple an expedient as either analyzing a thing more finely or analyzing a bigger thing, and since it is quite likely that anything, a blob of axle grease, for example, even if it were as homogeneous as it looks, has in total, on this principle, just as much structure as any other thing of the same size, a mouse or a watch, say, it seems hopelessly out of touch with what Aristotle or any other rational being could intend by his comparison of more and less form. Some improvement is made by stipulating that by "number of relations" we mean number of *kinds* of relations, for our homogeneous grease would anyhow include no such variety of relations as the mouse or the watch. Perhaps in some further reckoning of complexity we should find fruitful the almost virginal calculuses developed by Professors Goodman and Kemeny.[6] On any of these

[6] Nelson Goodman, "Axiomatic Measurement of Simplicity," *Journal of*

accounts, however, the result would remain, that the maddest chaos would have more form than a cosmos, which is so repugnant to normal ideas and to Aristotle's that we are tempted back by contrast to the opposite principle on which structure is proportioned to simplicity, for we sometimes praise works of art, at any rate, as if to have "more form" consisted of having plainer lines, less variety, and fewer relations. This, however, is the criterion of abstractness we have already had to reject. Aristotle praised circular motion under the impression that it is the nearest thing to no motion, but he did not praise the circular form as the next thing to nothing.

Now we all know that what is called "formed," "structured," "patterned," "configured," in a prerogative sense, is often denoted also by such epithets as "organization," "harmony," "coherence." Cosmologists, biologists, and aesthetes seem to agree with common sense in an intuitive cognizance of something of the sort, and modern idealists have been sure it is the chief criterion by which Aristotle assessed his degrees of form. It may be that all this verbiage is addressed to no one objective trait but only to a miscellany of vague lures, or anyhow represents something altogether alien to both simplicity and complexity of structure. On one prominent diagnosis, however, organization is composed, in a way easier to mention and observe than to explain, of a combination of complexity with simplicity, simplicity in complexity, unity in variety. As it affects properties this trait was described by certain idealists as "completeness and coherence" or differentiation and integration, and named "individuality" and "the concrete universal." As it affects propositions, what seems essentially the same thing was described by Leibniz as the principle of minimum antecedents for maximum consequents, and is still prescribed for scientific system as adequacy and economy of either terms or propositions or both. Among articulate ideas I know none which comes nearer to the feel of Aristotle's doctrine. Even his ecstatic attribution of "unity" to form, and especially to the essential form, is joy neither in mere relatedness,

Philosophy, LII (1955), 709-722; John G. Kemeny, "Two Measures of Complexity," *ibid.*, 722-733. S. F. Barker suggests other employment for them in *Induction and Hypothesis* (Ithaca, N.Y., 1957).

which adds up to complexity, nor in sheer oneness, which is paucity, but celebrates unity in multiplicity.

I still think that the only tolerably strict sense of "more structure" is complexity, conceived as some function of number, variety, and degree of constituent relations. But I shall take advantage of the historical and sentimental justification for using "higher form" or "amount (or degree) of structure in the special (or honorific) sense" to stand for organization, tentatively understood, or half-understood, as complexity-cum-simplicity, even though we have no notion about such essential factors in it as how much complexity in one dimension is equipollent with how much simplicity in another.

We are to see next, then, how well any of these notions of prerogative form match with the dignities which Aristotle arrogated for it. The dignities, I think, are best distributed under three categorial heads, the epistemic, the "ontic," and the normative. Epistemically form is the preeminently intelligible, intelligible in itself and imparting intelligibility to whatever else is intelligible. Ontically it is the preeminently actual, determinate in itself and determinative of other beings. Normatively it is the good, not merely the desirable but the desired and striven for. Our question then is how much these traits have to do with each other or with any of our accounts of "form" or with any one principle which is not merely their logical conjunction.

Philosophers, being professional understanders, naturally reckon understandability the foremost of the formal prerogatives, not always, to do them justice, mere de facto lucidity, but what Aristotle called being "knowable by nature," the objective complement to the principle of intelligence. Right off, and in so far as we understand intelligibility itself, it does seem that "form" in nearly every sense is correlated with it. The mind is more at home with the abstract and universal than with the concrete and particular, and among the abstract universals the structures seem more congenial and accessible than the qualia, and among structures of a given degree of complexity the organic ones seem more so than the rest. A second look does not annul but it modifies and diffuses this confidence.

There is no clear ontological principle which decrees whether a color quale, for example, whose formal rank is low, is less abstract and universal than the simplest single relation, and to Aristotle, Kant, and us, though not to Descartes, Spinoza, and Leibniz, it seems more abstract and more evident than even so primitive a structure as a triangle. More disconcerting is that among structures proper the criterion of intelligibility seems to give the palm to the simple and indeterminate, and for an uninspiring reason. What special intelligibility attaches to universals is a function of the high abstractness of the only universals which interest us, namely, the ones which are repeated; the total concrete character of Napoleon, for example, on the other hand, which is no less a universal for having only the one instance, is intellectually as unassimilable as the particular man, and is indeed the main reason why he is unassimilable. And the reason the simple and abstract is more comprehensible than the comparatively concrete seems to be only like the reason why a rabbit is more edible than an ox—there is not so much of it. The more complex forms, on the other hand, which alone we granted are strictly more form, are by no means better comprehended—nothing could be better comprehended, by God or man, than a simple quale or relation; their epistemic virtue seems to reside solely in that when we do comprehend them more is being comprehended and more comprehension is going on. We see then, darkly, how an amalgamation of simplicity with complexity, identity with diversity, integration with differentiation, which we called "more form" in a special and composite sense, would constitute "more intelligibility" in a similarly special and composite sense, the moment of simplicity contributing just to the intensity of comprehension, the moment of complexity constituting just the amount comprehended. This sounds too magical for comfort but does seem grounded in facts of experience and thought. Even the abstract universal notoriously performs some of this illuminating function of unity in diversity, in that its simplicity, which makes it perspicuous, is proportioned to its generality, which is its permeation of a various manifold of instances. A similar consequence follows, we may suppose, but more abundantly, on the so-called concrete universal, which the

idealists perhaps wrongly considered not analyzable into abstract universals but which is anyhow some sort of fugue of identity and diversity.

What has been said so far about "understanding" has concerned only understanding *what* something is, rather than *why* it is so. The latter strikes us as more important than it did Aristotle, who otherwise would never have called the form of a thing its "cause" at all, nor tried to assimilate to it the truly explanatory causes, efficient and final. We see readily enough how the "form" of a thing, in the prerogative sense, its essence, is supposed to impart intelligibility to such features of it as follow from the form, for to know what a system is may well be the same as knowing why each member is, and if "following from" the form is not the same as being included in it, it is probably near enough to share in the benefits. This, however, leaves the explanatory power of prerogative form narrowly limited. One limitation is a fact about the nature of organization. That a thing is itself organic does not in general assist it to have an organic explanation; on the contrary, a coherent content is as likely as not to make it incoherent with its context. What we usually think of as "the explanation" of a thing, moreover, is not the whole system it enters into but some salient other factor therein, which we call elliptically "the" cause or reason for it, and there is no implication that this cause should be especially organic either. Another sort of limitation is peculiar to the kind of forms Aristotle preferred. For he turned his back on the mathematical physicalism of Plato, the very prototype of system, paid its finest of structures only the monstrous compliment, "intelligible matter" (1036a 11), and cultivated instead the musky and raffish morphology of living things. This preference was bound up with his most remarkable restriction on the scope of organic system in general, his compartmentalizing of the world in his cardinal doctrine of primary substances separated by structural vacuum. Because he would not allow that any form could cover all the elements of even a single substance, the thing must have characters not intelligible from the one essential form, and because he made the essential form jealously exclusive, a principle of disunity as much as of unity, what is not intelligible by the main essential form cannot be

intelligible by another form or associated system, but must be left out in the cold of accident altogether. And again, because every form stops at the frontier of the middle-sized object which embodies it, the principle of form is automatically excluded from yielding any reason why for the substance itself and its form —it can't be a constituent in a more inclusive system as idealism and science aver. Since Aristotle has no explicit explanatory principle except form, however, he must have individual forms somehow explain themselves. He can't long stomach his trial notion that to know the essential what *is* to know the why (*Post. An.* 94a 1, 983a 27), and he is careful not to say that the single occurrence of a given form is cause of itself. Instead he says, over and over, that one occurrence of the form must be caused by another occurrence of the same form. And this causation, since it cannot be inclusion in a system, he must describe by the coarsest of common sense, as a sort of contagion, either like the infection of humanness which the parent transmits to the child by being his source, or the infection which manhood exercises on him by being his goal. These prima facie notions were poisonous to science in both their lessons, that the world works by imitations and by solicitations. With mention of them, however, we are trenching on the ontic and ethical prerogatives.

The belief that degrees of form constitute degrees of actuality seems more absurd than it is if we take it to connote degrees of existence, for of that notion Aristotle seems to be guiltless. If form has more actuality than its rivals, it has it, not by having more being, but by *being more*. Now the notion of sheer unspecified quantity, not of more this or more that, but just more, is not a completely silly one, though it is difficult and impractical, and not easy to reconcile with Aristotle's teaching that quantity is sporadic and secondary. The sum of a horse, an earthquake, and a prime number is in this pure regard just *more* than the sum of the horse and the number, and though we have no way of measuring one such amount against another, to compare them when neither is a proper part of the other, as Newton could deem mass to measure the net amount of matter in heterogeneous aggregates, I for one am ready to profess some intuitive sense for quantities of being in general.

It seems clear that when Aristotle asserted the superior actuality of form he did not intend the crudest such comparison, that the world's total quota of form outweighs the total of nonform. Nor did he mean merely that a form contributes something to the being of a thing, since this is compatible with its being quite inconsiderable. The best we can propose is that he meant that in any concrete existent there is more actuality invested in any form than in its matter, or anyhow—and this is the one workable rule till we are clearer about matter—that the higher the degree of the form, the greater its ontic weight. Without this last revision the proposition runs too counter to our ontological instincts to get a fair hearing. The matter or stuff of things, we feel, is what matters, what makes them actual, gives them content and impact. Their form is a frill; in most logical constructionist systems the relations of the elements don't count as entities at all. We may get over this impression but it must be by easy stages.

That prerogative forms, at any rate, are of superior actuality, is best defended for "more form" in the strict sense, which makes it accord with the Aristotelian doctrine that actuality is determinateness. The only independent way of understanding determinateness is by examples: being a disc, which is the conjunction of roundness and flatness, is more determinate than roundness; and scarlet is in an analogous though somewhat different sense more determinate than red. Aristotle, who would tell us that the species, rational animality, is more determinate than the genus, animality, which we should suppose true in the first sense, seems to have thought it true rather in the second sense—hence his remarkable, and remarkably popular, ukase that the differentia, rational, is equivalent to, and hence supposedly as determinate as, the species entire. But it would be hopeless to debate this now, so we must be satisfied to construe the superiority of the determinate as at least tantamount to the superiority of the complex and comparatively concrete. Thus edited, again the principle seems to me quite credible though not obviously important. If there is more being, absolutely, in the sum of a brick and a shingle, both ensconced in a house, than in either in abstraction from the other, there is surely more being in the sum of the color and shape of a tomato, say, than in either character singly. This is confirmed

by the common modern view that a thing consists of its characters, and that its complex characters consist of simpler ones, as well as by the perennial tradition that there are degrees of perfection constituted by number of predicates, heading up to God, the Ens Realissimum, who "formally or eminently" includes all positive attributes. That reality is proportioned to concreteness is one of the articles on which Positivists, Hegelians, and Peripatetics can agree.

Let us ignore the immense difficulties of applying such a criterion, to compare determinateness in different classificatory lines, or to allot relative weights to such factors as number, contrast, intensity, and extent of characters. More serious is its failure to give any advantage to structures over qualia, unless there be one in the fact that every added quale entails a round of additional relations to bed it down with the rest. Serious too now is the question whether any two concreta are not equally determinate in their total characters. The Aristotelian tradition seems to me to return two contrary verdicts on this, being committed to equal determinateness by the doctrine that no genus can occur without one or another from a gamut of mutually contrary differentiae, but committed against it by the doctrine of the scale of perfections, in which man's rationality, for example, is a sheer addition to the animality of the brutes, and not just our substitute for something they have and we don't. Most serious, however, is that it involves that equation of superior form with mere complexity which we found as incompatible with what seem to be Aristotle's main intentions as we did its equation with abstractness. Why not infer at once, then, that Aristotle believes that superior actuality is constituted by "more form" in the special sense, complexity-cum-simplicity? Quite likely he does. The trouble is, however, that this identification must either be interpreted out of any normal meaning or remain staringly incredible. It took some schooling to make us think that if ϕ is more determinate than ψ, then ϕ has more reality than ψ, in anything like an ordinary or important sense of "reality." It seems impossible to believe honestly that if they are equally determinate, but ϕ is unified and ψ is not, then ϕ has "more reality," except in a trumped-up new sense in which "reality" *means* organization.

Aristotle himself can only vacillate, and centuries of Aristotelians have swayed back and forth in his train. He is loyal to determinateness when he envisages that the conjunction of the accidental forms with the essential form composes a greater sum of actuality or perfection. He abandons it, however, when he denies that the essence *white man*, which certainly is more determinate than *man*, is more of a form than the latter (1029b 25 ff.). This is not because whiteness is not a structure, for the same is to be said, apparently, for *snub nose* (1030a, b; 1038a), and *snub* and *snub nose* are structures. And it can hardly be for the reasons he alleges, which seem to be both irrelevant to the problem and inconsistent with each other: that *white* is independent of *man* and that *snub* is not independent of *nose*. The ground for such compunctions might be, however, that the extra adjective adds to the determinateness, making "more form" in the strict sense, but detracts from the organicness, making "less form" in the special or honorific sense. We might say, then, that determinateness and organicness constitute "more reality" in the strict and honorific senses, respectively, but the Aristotelian can hardly say this without giving away that the extra reality of the organic is only its organicness; we must leave him in his quandary.

Philosophers of John Dewey's party have alternately denounced Aristotle as the advocate of static substance and structure, and extolled him as preeminently the philosopher of dynamic function. And indeed Aristotle sometimes outdoes Dewey: the actual house is identical with the construction job which makes it (1032b 13, 1070b 34), or with the use it is subsequently put to (1043a 16)—with anything rather than the stolid edifice which we should think had to mediate between those adventures. And though such utter instrumentalism, identifying a thing with what is done to it and with it, and even with its effects on and in other things (202b 5), is not always maintained, Aristotle insists at least that to *be* more is to *do* more. "Actuality" (ἐνέργεια) in the original sense, he says, is movement (1047a 32); and the word still means being at work, on the job. The terminology of Latin Aristotelianism, I gather, deliberately cherished the ambiguity, to keep it difficult to distinguish between *actuality* and *activity*, so that their kinship took on the look of a cosmic necessity.

In fact, however, few ties in the system are flimsier than this. In none of the senses we have surmised for "form" and "more form," "actual" and "more actual," is there any intrinsic connection, nor much extrinsic or empirical connection, between them and activity, whether we mean by "activity" causal efficacy or motion. The first Deweyan party are right that the main orientation of Aristotelianism is such as to oppose actuality to activity. In spite of some sour allusions to the concept of rhythms in Democritus (985b 16), Aristotle justifies his modern partisan's boast that his philosophy cannot admit forms of process but only a process of forms.[7] What he thinks to be formed and actual in the highest degree has neither motion nor efficacy; the primary mundane realities are substances "ferociously independent" (as Professor de Wulf has put it), whose essences are real in proportion as they emulate the changeless forms and which cannot be in organic interrelation with one another. For this reason, which has already come to our attention under the head of intelligibility, motions and changes and their commerce must be surd violences by which one form is exchanged for another form, or a form in one thing migrates into another thing—vulgar notions we found alien to the formal system. So far is it from self-evidently necessary that a high degree of Aristotelian form and actuality must surge into motility and influence that Aristotle rails at Plato that qualia are dynamically prior to structures, that form has efficacy only when it is beefed up with matter (990a 10, 1033b 26), and indeed that motion and the power of

[7] W. O. Martin, *The Order and Integration of Knowledge* (Ann Arbor, Mich., 1957), p. 20. I am afraid that Professor Martin is mistaken in his belief that Whitehead's philosophy of eternal objects is in different case in this respect, since change for him too turns out to be a succession of forms. Whitehead did, however, have at least the benefit of organicism, by which successive forms can and must *con*form. The monadism of Aristotle provoked the Thomistic existentialism, the idea of "the gush of Being" per se, neither form nor matter. "Devoted to the mystery of the act of existing, this philosophy is by that very fact devoted to the mystery of action and the mystery of movement" (J. Maritain, *Existence and the Existent*, New York, 1948, p. 45). By this strenuous innovation, however, the incoherence is only transferred and blurred. Let actuality be affiliated with form, with existence, or with mystery, as you like, nothing is going to make it entail either efficacy or motion, any more than any hypothesis or paraphrase can make music entail bimetallism.

being moved reside in matter. There would be not less but more form and actuality in an Aristotelian world where nothing either moved or influenced anything else.

Turning from what Aristotle's general philosophy inculcated about these topics to what seems established today as the truth about them, we find a somewhat closer connection among structure, actuality, and activity. A motion is a structure; the principal causal linkage, the main executive order of nature, is one which involves the chronogeometrical structure of things rather than their qualia; and by and large the greater aggregates of being have more effect than the slighter. It is only an empirical fact, however, that the world confesses to being rather a Platonic frame of chronogeometry than an Aristotelian broth of qualia, or that elephants are stronger than mice, and not even in empirical fact is there any conspicuous correlation between activity and "more form" in the strict sense, and there is none, I guess, between activity and "more form" in the special sense.

The third prerogative which Aristotle assigns to form and essence, that this principle which is the intelligible and the actual is also the desirable or valuable, would be ruled out a priori by certain philosophies now current which sunder *ought* from *is* by a chasm of linguistic use, but since I think this easy mode of dismissal is less likely to be valid than almost anything in Aristotle, it is not open to me. On the contrary, the proposition that entities are intrinsically good in proportion as they are "formed" seems as credible as any doctrine in moral philosophy—"formed" not merely by being abstract and universal, which Plato and Santayana revered, but by being more abundantly and determinately actual, and especially by being organic. It is good for a person or any living thing or anything whatever to be differentiated and integrated—perhaps the only good and perhaps the analytic content of goodness itself. But no proposition in moral philosophy is very credible, and this is not, and Aristotle really gives it little plain statement or support. He explains that the forms which constitute goodness are the essential forms, and then that the essential forms are those which constitute goodness, with no clear indication how this sense of "essential" is going to coincide with others. We suspect that in fact only some forms, and only some

organic forms, are good, and that we associate goodness and form by that same normative bias which uses "quality" for good quality. There is much also to make us suspect the converse, that only some goods are forms. Hedonists say that goodness is identical with pleasure, which is a quale, and though G. E. Moore has recanted his old thesis that good is some other sort of simple unanalyzable quality, he is no more infallible now than then. Many a thoughtful man, finally, believes that individuation, and hence, in the Aristotelian system, prime matter, provides the ultimate good. True or false, therefore, Aristotle's identification of form with the good is another additional and independent principle not deducible from the rest of his conceptions, but only, as it were, *becoming* to them.

That living things tend toward the good and toward good form, not in the sense that they regularly wind up with it, for they don't, but in that they prefer it, is again eminently plausible. But again the doughty Aristotle, in the *Ethics*, relies on little but circles of definition to defend it—for example, that the essential tendency of a thing is toward its true good, its true good constitutes its proper happiness, and its proper happiness is the realization of its essential tendency. In the world outside these verbal reciprocities there are psychological facts and theological tenets which suggest that men, at any rate, are naturally prone to evil, and much in biology and cosmology which suggests that the appearance of a vital teleology is just the circumstance that configurations of the sort which causally preserve and propagate themselves do preserve and propagate just those configurations, and that even organization and unity-in-variety are only incidental to this, and not closely proportioned to it either. Even the appearance of such propensity stops, anyhow, at the frontiers of life. Science agrees with the Platonic theme again, that physical reality tends rather to disorganization than organization. Aristotle's animism, having taken its root metaphor holus bolus from the world's infinitesimal minority of life and manufacture, could keep from being unmistakably fantasy only by two devices of somewhat contrary import. One, which won't do at all, is that most of the universe, the non-living part of it, is not really real, does not compose actual substances, but is mere "crud" as the

boys say; the other, which is a fair philosophical tactic, is that despite appearances everything is more or less alive.

It is not enough teleological amendment of the doctrine of form, however, to suggest that efficient causes, including plans in men's minds, happen to converge to increase organization. Socrates, Plato, and Aristotle would rather that mind and value were impotent and forlorn altogether than that they act and be aimed at by efficient causation. So Aristotle's finalism must involve an ontologically unique tension toward the formal "end," τέλος—words which fatefully for Europe compounded the diverse notions of aim, determinateness, and cessation. Beyond saying that such telic tug is an additional ultimate in the Aristotelian philosophy, however, I despair even of adjusting it to the general scheme. For a Platonist who believes there are disembodied Forms, and for an actualist who believes that the future is real, these can provide sources of attraction, goals for purpose, but for Aristotle, who believed in neither, the final cause must be sheer nothingness till it is realized and then it is not a final cause. If there is any formal target meanwhile it has to be embodied in some other current instance of the form, or, by the most astonishing excrescence of all, it must be other and superior forms in a hierarchy up to God. Aristotle has no evidence either that there are forms superior to man, or that bullfrogs and olives are emulating man, or in general that things can achieve their own forms by aiming at superior forms, except that if we make all these assumptions we may explain things teleologically. It remains unexplained *how* one form should lure a different form into existence, and especially how this is compatible with a philosophy in which species are atomic fixed types, so that it is blasphemous or nonsensical that anything should aim at any other form than its own; and how, in particular, the rout of mundane things can be said to imitate the essentially motionless form of Divinity by all moving like the devil.

(To be concluded)

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NECESSARY STATEMENTS AND RULES*

I WANT to explore some relations between certain kinds of necessary statements and certain associated rules of language. If possible, I would like to clarify the non-deductive certification of necessary statements—for not all necessary statements can be certified as valid by deduction from necessary statements previously accepted as valid. We can always try to prove a given necessary statement, but the attempt may fail because the statement in question is in some fashion too "simple" or "fundamental" for proof to be feasible. If this is so, we are not driven to the dogmatic assertion of the necessary statement without supporting grounds; nor need we await the dubious blessing of some supposed act of infallible intuition. There are non-deductive ways of certifying necessary statements that provide rational grounds for their assertion; where the resources of proof are exhausted, there remain other discursive tests to be applied.

Such certification, as it may be called, has less practical importance than recourse to proof. Scientists and mathematicians (not to speak of the "man in the street") have been trained to accept some simple necessary statements without hesitation or question; their problems about necessary statements are always problems of calculation. A philosopher, however, will probably be more perplexed about certification than about proof—though he is interested in both. For unless he can satisfy himself that he has a sufficiently clear understanding of how some necessary statements are certified, he will naturally regard the process of proof as showing merely how some necessary statements depend upon others.

I wish to argue that the process of certification may properly be regarded as one of checking a necessary statement against a corresponding rule of language. Or, rather, I shall argue that this is so in the case of a special class of necessary statements. I want

* A shorter version of this paper was delivered as a "Special Lecture in Philosophy" at London University, February, 1955.

to show how, and in what ways, such necessary statements are based upon and function as surrogates for linguistic rules. But these expressions, "based upon," and "function as surrogates," are at present too imprecise for useful discussion; and the same might be said for the familiar word "rule." The success of the following sketch will depend largely upon the extent to which these expressions receive relatively more precise meanings.

(It will have been noticed that I have chosen to speak of the "validity" of necessary statements rather than of their "truth," and of their "certification" rather than their "verification." My purpose in so doing has been to emphasize from the outset the differences between necessary statements and contingent ones. If the theory to be outlined below is sound, it will become plain that the uses of necessary statements differ strikingly from those of contingent statements. So great are these differences that a good case might be made for rejecting the expression "necessary *statement*" altogether. Unfortunately, if we withhold the title of "statement," we shall have no good alternative label. "Axiom" might serve if it were not already pre-empted in special senses. The best we can do is to continue talking about "necessary statements," without allowing the label to control our thoughts.)

I

I wish to conduct the discussion by using a single example of a necessary statement that can be certified but not usefully proved. In order to develop my case as simply as possible, I want to exclude statements belonging to logic or pure mathematics — like "$(p) : p \vee \sim p$" and "$1 + 1 = 2$" — and also statements in which particular words occur "vacuously" (like "A man is a man" or "If Tom is an Englishman, Tom is either an Englishman or a Frenchman").

Consider the following statement, which I shall call "*S*":

Monday is the day before Tuesday. (*S*)

I fear there may be some reluctance to accept this as an authentic necessary statement, on the ground that it refers to an artificial

division of the week, and is therefore a mere "convention" or "definition." I think such objections are unsound for reasons that I do not now propose to discuss. But in any case I do not wish to argue about the choice of an example. I hope that what is to be said will apply equally well to the case of any other simple necessary statement that might be substituted for *S*. (However, the example ought to be one that would normally be called an "analytical" necessary statement, and not the sort that would commonly be called "synthetic.")

Given the necessary statement, *S* ("Monday is the day before Tuesday"), we can now construct what I shall call the *associated linguistic rule*, or "*R*" for short:

> "Monday" may be replaced by "the day before Tuesday" and vice versa. (*R*)

If we are given any necessary statement whose verbal expression consists of some expression (or word) E_1, followed by the word "is" and then by an expression (or word) E_2, so that the whole necessary statement reads "E_1 is E_2," the associated linguistic rule will be to the effect that E_1 can be replaced by E_2, and also E_2 by E_1.

II

My chief contention will be that *one* way to certify *S* is to establish that *R* is indeed a rule in force in the English language. But now is there such a rule of synonymity concerning the word "Monday" and the expression "the day before Tuesday" in the English language as at present spoken? An affirmative answer would have the following important consequence: Anybody who substituted the word "Wednesday" for the expression "the day before Tuesday" would be criticized for making a mistake, would be called to account as soon as the substitution was noticed. Linguistic rules are normative; they state what is and is not to be done when the language is used; and so departures from or violations of the rules evoke complaint and demands for correction.

The same, however, can be said of some misstatements of fact. Anybody who said "London is the capital of the United States"

would also be regarded as making a mistake and would be held to account as soon as his remark was noticed. So the fact that anybody who replaced the expression "the day before Tuesday" by the word "Wednesday" would be held to be wrong and called upon to retract, does not yet show that we are dealing with a rule of language. We have to decide what reasons would be offered for or against such deviations from normality and the sanctions that would ultimately be invoked if the offence were repeated. If a man said, in apparently good faith, that London was the capital of the United States, we would suspect that he was speaking figuratively or, possibly, making some paradoxical statement with a hidden point to it. But if he were to insist upon repeating it, while maintaining that he meant just what he said, we could only assume that he himself did not believe his own words and was, for some obscure purpose, sowing confusion in his hearers. But if a man insisted on substituting "Wednesday" for the expression, "the day before Tuesday," and proved equally refractory to attempted correction, our conclusion could only be that he was using some of the words in senses differing from our own. This kind of behavior, I am maintaining, in which the man in question insists on making unorthodox substitutions, is conclusive evidence that he is using some words in ways other than those prescribed in the English language. It is along these lines, I believe, that we can distinguish the case of a violation of a linguistic rule from that of an error about fact.

Consider the corresponding situation in connection with an explicit rule of definition that somebody adds to the language. If I introduce the expression "Old Year's Day" for the last day of the year and subsequently find somebody who is willing to substitute the expression "Old Year's Day" for, say, the first of December, that is conclusive proof that the other man cannot be using my label in the way I had intended. The situation with respect to "Monday" and the expression "the day before Tuesday" differs inasmuch as nobody known to us has introduced the word "Monday" into the English language and in that no unique stipulative definition of that word is available. But this is no reason for denying that there is a rule of synonymity governing the use of the two expressions.

I am going to take it as established in what follows that *R* is a rule of synonymity now in force in the English language, and I shall proceed to examine the relations between *R* and the necessary statement, *S*.[1]

III

The chief thing I wish to say about the relations between *S* and *R* is that a necessary and sufficient condition for the validity of the former is that the latter should be a rule now in force in the English language.

This view about the certification of necessary statements may seem reminiscent of theories that have been called "linguistic theories of the a priori" or "conventionalistic interpretations of necessary statements." Such theories have in the past been exposed to a number of powerful objections which I must try to meet in their proper place. But supposing the objections can be met (as I hope they can), what positive grounds can be offered in support of the contention? I can think of no better way of persuading somebody who is not already convinced than by asking him to consider what would be the relation between a statement of the form I have been discussing and the associated linguistic rule of synonymity, in case there should be such a rule in force. If we were to agree that the word "flubor" should in future count as a synonym for the expression "female fox," would not the statement "A flubor is a female fox" then be a necessary statement? If the answer is in the affirmative, the defense of my contention will then shift to the question of whether there are rules of synonymity that have not been introduced into the language by explicit definition. But about this I have already said all that I can at present.

(In any case we must remind ourselves that philosophical conclusions do not stand to their reasons as the conclusion of a deductive argument to its sufficient premises. The success of any

[1] Some readers are going to demand a good deal of argument before they are satisfied that *R* is a rule now in force in the English language; others will perhaps need convincing that natural languages contain any normative rules or regulations. A discussion of the notion of a linguistic rule that might persuade these dissenters would require so large a digression that I have decided to reserve the topic for a separate paper.

philosophical investigation must be judged by the extent that it dispels initial perplexity and permits a perspicuous view of a complex territory. The value of any "theory" about necessary statements must be judged in the end by the light it throws upon the ways in which necessary statements are used, their modes of certification, their relations to contingent statements, and so on. The choice between the view I am trying to develop and, say, the "realist" interpretation will be settled in the end by such considerations as these and not by formal argument for or against one or the other view.)

IV

I have claimed that *S* is certified, that is, shown to be valid, by establishing that the associated linguistic rule, *R*, holds in the English language. In other words, to certify *S* is to verify the contingent proposition, *E*, that *R* is a rule of the English language. It is to be noted, however, that I am not identifying the necessary statement, *S*, either with *R* or with the contingent statement *E*.

Other philosophers who have defended views about necessary statements similar to those I am here presenting, have sometimes made the mistake of identifying a necessary statement with a convention, definition, resolution, or the like. Thus Professor A. J. Ayer once incautiously said of a necessary statement that it "*records our determination*" to use words in a certain way.[2] Professor C. D. Broad thought he could refute this by saying "If an analytic proposition states that the person who records it intends to use certain words in a certain way, it evidently makes a statement about the present experiences of the speaker, and about his future behaviour."[3] And he went on to say that this view treats all analytic propositions as empirical and synthetic. It is, however, a mistake to treat a resolution as the conjunction of a report about present experience and a forecast about future behavior. And so Broad's refutation of Ayer must be rejected. And yet it *would*, for all that, also be a mistake to say that a necessary state-

[2] *Language, Truth and Logic* (London, 1936), p. 104.

[3] *Proceedings of the Aristotelian Society*, Supplementary Vol. XV (1936), 107.

ment *is* the same thing as a resolution (a "record of our determination" to use words in a certain way). Quite superficial considerations are enough to show that necessary statements are not used in the way that statements of resolutions are used. We can, for example, always ask of a given resolution who were its authors and when the resolution was made, yet these questions are nonsensical when applied to a necessary statement. No sense attaches to the question, "*Who* resolved that two and two shall make four?" or the question "*When* was it resolved that two and two should make four?" Again a resolution about words must involve reference *to* the words, yet the corresponding necessary statement is not about the words but rather *uses* those words. ("I resolve to to use 'two and two' and 'four' as synonyms" mentions the expressions "two and two" and "four"; the statement "two and two make four" *uses* those same expressions.)

It is easy enough to bring parallel considerations against any view that identifies necessary statements with definitions or conventions or rules of language. Very little effort is needed to show that the grammar of "necessary statement" differs at crucial points from the grammar of "convention" or "definition," or the other terms by which it has been attempted to be replaced.

Yet it by no means follows that in *certifying* a necessary statement we cannot be establishing that an associated rule holds in the English language or, for that matter, verifying a corresponding empirical statement to the effect that the rule holds. Consider the following analogy. In order to make sure that a given check is good, we have to verify that the account on which it is drawn has sufficient funds to meet it, and this is a matter of fact. But it by no means follows that the check itself is the expression of a matter of fact, say an assertion to the effect that the account in question does contain sufficient funds for the draft to be honored. The check is an *order*, even though the determination of the worth or value of that order is a matter of empirical verification.

Or consider the case of an Act of Parliament. That the Act is valid or legitimate can be established by examining appropriate documents and, if necessary, settling questions of historical fact (whether the Bill was properly read, whether a proper Parliament was sitting at the time, and so on). But the Act itself is not a

collection of historical statements; that the determination of the legitimacy of the Act is an empirical inquiry does not make it necessary for us to say that the Act itself is an empirical statement or a collection of empirical statements.

Now much the same can be said about many statements that are not themselves statements of fact. In many such cases the statement in question can be *appraised* as "correct" or "incorrect," "valid" or "invalid." And the statement of appraisal may itself be checked or "verified" by appeal to matters of fact. Yet it never follows, on this ground alone, that what is appraised is itself a statement of fact. Nor does it follow that the appraisal is a statement of fact.

V

The best way to make clear what the relation is of the necessary statement S to the associated linguistic rule, R, is, I think, to review some of the main uses of necessary statements. I propose to deal with this somewhat indirectly, by considering first some of the distinctive uses of rules in a system constituted by the presence of those rules. And instead of considering the uses of linguistic rules in language, I shall begin with the simpler case of the use of the rules of chess.

The rules of chess seem to be used in at least the following five ways:

(1) We formulate and pay explicit attention to the rules when learning or teaching the game.

(2) Once the rules have been learned, there are many of them that we never have occasion to consider explicitly. I doubt if any chessplayer ever has before his mind the rule that two pieces must not occupy the same square simultaneously; such a possibility, as we say, "never occurs to him." It seems that one effect of acceptance of the rules of the game is to reduce the indefinitely large number of physically possible maneuvers of the pieces to just that set which are characterized as "legal moves" in a given position. Thus no chessplayer ever takes account of the physical possibility that a piece will move right off the chessboard (indeed

this would not count as a "move"); the effect of his having accepted the rules is that this physical possibility is excluded from his calculations. We may say that the rules create a *Spielraum*—an idealized space in which only "legal moves" can occur.

(3) But occasions may occur when the legality of a proposed move becomes problematic. Chessplayers sometimes doubt whether a King may castle by passing over a square controlled by an enemy piece (partly because occasions for the application of the rule are so rare that they tend to forget it and partly because the rule seems more "arbitrary" than some of the others). A player uncertain whether he is allowed to perform the maneuver in question may still in some sense "know" the rule. By enunciating the rule explicitly he makes it clear to himself that the move would be illegal.

(4) A similar situation arises when an opponent makes an illegal move. Then the relevant rule is recited to convince the offender that an infringement has in fact occurred.

(5) Explicit reference *to* the rules is needed if a theory *of* the game is to be constituted. For example, if I want to show that the maximum number of Queens on the board at any time is 18, I shall have to state the rules in order to draw certain inferences.

These uses of the rules of chess can be briefly but not too misleadingly summarized as follows: The rules in their entirety define the game of chess—to play chess is to play in conformity with, and with due recognition of, the authority of those rules. So, explicit formulation of the rules occurs, as we should expect, in learning or teaching the game (case 1) or in move behavior not already sufficiently fixed by the original teaching procedure (case 3), or in challenging actual or alleged infringements of the rule (case 4) or in making preparation for explicit analytic talk *about* the game (case 5). We might add, somewhat paradoxically, that the rules are most effective when no explicit or conscious attention is paid to them, their influence being then shown only in the habits that their acceptance creates and sustains (case 2).

Now let us see how far what has been said about the rules of a game like chess will apply, with suitable changes, to a language. To the rules of chess there correspond now the rules of meaning

that define a particular language. And we should expect that there would be occasion for the explicit enunciation of the rules when the language is being learnt or taught (case 1) or when the appropriate linguistic behavior is not yet sufficiently determinate (case 3) or when an actual or suspected infringement of the rules occurs (case 4) or in inquiries like the present essay in which analytical investigations are made into the theory of language. We must bear in mind that infringement of the rules now means failure to talk sense according to the conventions determining the meaning of expressions in the particular language in question. Speaking roughly, then, the function of the affirmation of the rules is to render explicit to ourselves or to others the meanings of expressions in the language we are using. As in the parallel case of the game, we have good reason to invoke the rules only in the cases where there is some indication or risk that the language is not being used correctly. The distinctive use of the rules of language is to inculcate, reinforce, and exhibit the meaning of statements in that language.

VI

I want to argue now that some of these distinctive functions of linguistic rules can be facilitated by the use of "dummy statements"and that necessary statements can be properly regarded in this light. There is nothing corresponding to this in the game of chess as it is played at present. As Frege said, "It is true that the moves of the game are made in accordance with the rules; but no position of the chessmen, and no move expresses a rule; for it is not at all the job of chessmen to express anything; they are, rather to be moved in accordance with rules."[4] (Frege goes on to say that *if* positions expressed rules, there would be danger of conflict between the arbitrary manipulations of chess positions and the restrictions necessitated by the need to transform positions expressing rules only into those expressing *inferences* from the rules.) But let us consider how the present game of chess might

[4] *Translations from the Philosophical Writings of Frege* (Oxford, 1952), p. 203.

be modified by the introduction of what I shall call "*dummy pieces*."

(1) Imagine that a new piece, the "Blocker," is introduced, its powers being defined as follows: "There is only one Blocker. At the outset of the game the Blocker is placed on a certain square to be agreed upon by the two players; it then stays on that square for the duration of the game. The Blocker can neither capture nor be captured." This would be a very inert kind of piece, and we should be strongly inclined to call it a "dummy." The word "dummy" suggests a waxwork man or a lay figure, resembling a living man in appearance, yet incapable of motion or action. More generally, we use the word to refer to a counterfeit object, a mere simulacrum or pretence. Now it is easy to see why the Blocker should be so regarded: it may look like a piece (be made of wood like the others and have a distinctive conical shape) yet it neither moves, captures, nor is captured. We might add that it belongs to neither side. And we might compare it to the function of the dummy fourth player in dummy whist. But it would be an over-statement to say that it does *no* work, for it serves exactly the same function as would a certain rule, viz., "There is one square, to be agreed upon by both players at the outset of each game, which may at no time be occupied or traversed by any piece."

It might seem easier to introduce this rule than to add the peculiar dummy piece; yet our imagined "Blocker" has at least a *mnemonic* function, for it is easier to stay off an occupied square than to remember an arbitrary agreement about keeping out of a square. The Blocker is about as useful as the crosses that builders paint on windowpanes to remind themselves not to crash through the glass.

(2) If the game of chess were ever to be modified in the manner described, nobody would be likely to treat the added chessman as if it were "*really* a piece" of somewhat unusual powers of movement. But now consider still another way in which the game might be modified: this time suppose each player to have a piece, called the "King's Companion" subject to the following regulations: "The King's Companion *may* be placed on a square

adjacent to the King immediately after the King has moved out of check, such placing of the King's Companion not to count as an extra move. The King's Companion *must* be removed immediately before the player makes his next move, such removal not to count as an extra move. The King's Companion may neither capture nor be captured."

Once again we have a "dummy piece" and, as in the last example, all that is achieved by the introduction of the new piece *could* be achieved by the addition of a rule suitably framed. The presence on the board of the "King's Companion" acts as a restriction upon the freedom of movement of the other pieces on the board, the presence and position of the obstacle to movement varying with the character of the position and the will of the players. And it would be possible to frame a rule imposing the very same restrictions upon the moves at present allowed in ordinary chess. But I think everybody will agree that the "mnemonic value" of the new chessman in this case would be very much greater than in the case of the "Blocker." I think it would be impossible *in practice* to observe a rule of the required complexity without the intermediary of the dummy piece. But just because this dummy piece, unlike the simpler "Blocker," has several resemblances to "regular pieces," I think there would be correspondingly more inclination to regard it as a "regular" piece with "queer" powers.

(3) Finally, imagine new pieces whose use in the game was still mainly to "block" or to restrict the freedom of movement of other pieces, but now in far more complicated ways. We might imagine each side to have two "Obstructors" whose relative positions on the board had to conform to complex specifications (for example, that two Obstructors of the same color must always be on the same rank or file), we might suppose that the "Obstructors" could force other Obstructors temporarily *off* the board, but could not capture regular pieces, and so on. By the time this amount of complexity had been introduced, the practical utility of these "dummy pieces" as surrogates for the rules that *might* have been introduced instead would be very great. And the variety of ways in which the new "Obstructors" could appear on

the board and the dependence of their positions upon the choice of the players would make it very plausible for them to be regarded as regular, though still somewhat "peculiar" pieces. For, after all, marked differences in function can be found in the pieces already in use (as is shown for instance by the privileged position of the King); and the peculiarities in the "moves" of the new pieces might not be regarded as too great to be tolerated in "genuine pieces."

Much of what has been said about the introduction of possible "dummy pieces" could be also said, with minor changes, about "dummy *moves*." Consider "two-move chess," played exactly like the present game, except that each player makes two consecutive moves in turn. And now suppose when it is White's turn to move he causes a piece to pass to another square and then return back to its original square, so that the position of the pieces on the chessboard is unchanged by his action. I think that a "move" of this kind might properly be regarded as a pretense, or counterfeit, or dummy—and for the very same reasons as led us to apply the label of "dummy" to certain *pieces*. The rules of "two-move chess," as I have imagined them, permit moves the second part of which reverses the effect of the first. Now instead of calling these self-cancelling operations "moves," we could define the very same game by agreeing (i) that only maneuvers resulting in a *change* of the position of at least one piece shall count as "moves," but (ii) a player shall have the privilege, when his turn comes, of choosing *not* to move. In other words, so far as the game goes, the effect of these "dummy moves" could be achieved by a mere change in the rules. This is not to say that there may not be practical utility in admitting dummy moves—as there was in admitting dummy pieces—for their introduction may lead to substantial simplification in the rules. Indeed, since a "dummy piece" may be regarded as one that makes only "dummy moves," much of what was said about the utility of such pieces will carry over, with but little modification, to the case of the dummy move.

One final remark about the game of chess as it is now played. Consider the setting up of the pieces at the beginning of the game. It is not usual to call this a move or a series of moves. Yet it

involves the manipulation of pieces in ways which are specified by the rules; we could therefore, if we pleased, call it a "move." But we would immediately want to qualify this description by saying it is a "very peculiar kind" of move. It is "very peculiar" because instead of setting up the pieces in their initial configuration we could imagine a new set of rules that would achieve the same end. Suppose the game were to start with an empty chess board, each piece appearing on it only when it first "did some work" either by moving, obstructing, capturing, checking or being checked, and in such ways that just the moves that are *now* legal in the game should be admissible. I say it is conceivable that a set of rules should accomplish this end without there being any initial configuration, but of course the practical difficulties would be staggering. It seems likely that the game would become so difficult that no person of average intellectual powers would ever be able to play it correctly. The function of the "dummy move" that consists in placing all the pieces on their initial squares is to render possible a relative simplicity of the rules. I hope this final example will show very clearly the practical utility of what might otherwise seem the artificial expedient of introducing "dummy moves" and "dummy pieces" into a game.

VII

Let us now see how the notions outlined in the last two sections can help us to understand the role of necessary statements. I have said that "the distinctive use of the rules of language is to inculcate, reinforce, and preserve the meaning of statements in that language." Consider, for instance, the rule "Wherever 'father' occurs 'parent' may be substituted." This can be looked upon as a succinct way of allowing certain moves in the "game" of inference, for example, from the statement "Tom is a father" to "Tom is a parent."[5] In this form the rule is clearly *about* permis-

[5] The actual rules connecting the uses of "parent" and "father" are quite complicated. We are not entitled to pass from "I saw the father of Mary" to "I saw the parent of Mary," for instance. But such additional complexity of detail will not hurt the argument.

sible transitions from one statement to another and is not itself a statement, as rules of chess are about the moves of pieces and are not themselves pieces. But if we take the corresponding necessary statement, "A father is a parent," we have something that might well be called a "dummy" statement. Counting it as a "statement" that may be used as a *premise* allows the rules of inferential deductive transformation to take a simpler form. Inasmuch as the necessary statement uses ordinary words combined in a familiar construction, we may well be inclined to accord it all the dignities that go with the title of "statement." But as we reflect that the rules of our language *dictate* the form of the necessary statement, so that *we* can convey no information by using it, we may perhaps feel inclined to qualify our description and to add "but it is a very peculiar kind of statement." I think necessary statements can be regarded as a "very peculiar kind of statement"—as dummy assertions whose chief function is to simplify the rules of inference.

That such simplification is produced by the use of the trivial necessary statement "A father is a parent" is not at all obvious. The full advantages of the quasi-statement form for necessary statements are most clearly seen when we calculate with necessary statements. Meanwhile, the kind of simplification we have in mind can be illustrated by the following simple example. Let us regard any necessary statement having the form "*p* or not-*p*" as a "dummy" assertion in the sense already explained. Suppose we already have a rule for the validity of the dilemma, that is, for an argument of the form

(α) *a* or *b*
if *a* then *c*
if *b* then *d*
$\therefore$ *c* or *d*.

Now let us suppose we have to determine the validity of an argument

(β) if *a* then *c*
if not-*a* then *d*
$\therefore$ *c* or *d*.

As presented, argument (β) does not have the logical form of (α) and the rule certifying the validity of the latter does not yet

certify the validity of the former. Suppose, however, we add to (β) the dummy premise "*a* or not-*a*," thus obtaining

(α') *a* or not-*a*
if *a* then *c*
if not-*a* then *d*
$\therefore$ *c* or *d*.

Now (α') does have the same form as (α) and one and the same rule certifies both. We no longer need to find a separate rule for (β) and have reduced the number of types of arguments that need to be treated as separate or exceptional cases.

VIII

In the last section I treated assertions as if they corresponded to pieces and transitions from one assertion to another as if they corresponded to moves in the "game of inference." Another way of looking at the matter might be to regard an assertion itself as a move in the "game of using language." In that case certain grammatically well-formed statements might, from yet another standpoint, have to be regarded as dummy assertions. We have already considered a kind of "move" (in a game) that left the position unchanged, and were therefore called "dummy" or quasi-moves. In order to treat necessary statements as "dummy" assertions, it will be necessary to show how they can be regarded as making no difference to the "game" of assertion.

I shall lead up to this by considering a number of instances, rather like this one of the empty move, of what I shall call "self-cancelling operations."

(1) Suppose we have a simple code for instructing a person to reach an indicated destination, the manner in which he reaches the end point being unimportant. Let the instruction "N5E2S1" mean that he is to proceed to a point, in whichever way he chooses, that *can* be reached by first going five miles north, then two miles east and finally one mile south. Every instruction, accordingly, begins with the indication of a compass point, is followed by a number, continues alternately with compass points and numbers,

and ends with a number. (The foregoing might be regarded as a concise way of specifying the syntax of the code.) With this understanding what are we to say of such an instruction as "$N_5W_5S_5E_5$"? In order to obey this, the receiver must go to a point which *could* be reached by going first five miles north, then five miles west, then five miles south, and finally five miles east—in other words, he is asked to "go" to his starting point. Shall we call this an "instruction" after all? Since the receiver can "conform" by doing nothing at all, the effect of understanding and "performing" this instruction, if we are to call it that, is the same as that of not having received an instruction at all. So we might be inclined to say it "really" is *no* instruction. But if we do so we shall have to reformulate the syntactical rules of the code in a very complex fashion, for the description of the combinations of letters and numbers that would not be admissible, though it could be given, would be very complicated. It is simpler in practice to allow a *redundancy* in the code—that is, to permit sequences such as "$N_5W_5S_5E_5$" to *count as* "instructions" and to be transmitted. For the receiver to understand this partially redundant language in which instructions are transmitted, it is clear that he must understand that the instruction "$N_5W_5S_5E_5$" makes no demands upon him. It is a dummy "instruction"—or, as we might prefer to say, a "limiting case" of an instruction.

(2) At a certain restaurant the clients order dinner by crossing out those items on the menu that they do not wish to have served. One evening it happens that a customer perversely chooses to cross out *all* the items of the menu; shall we say he has or has not "ordered" a meal? Clearly, this is a question of how we shall choose to *describe* what actually happened. The chef would probably say no order had been given, while a mathematician's predilection would be for saying that a "null order" or a "limiting order" had been given. The imaginary situation here described may be considered as involving a primitive "language" where "statements" are produced by striking lines through items on the list of articles available. The question whether an act of striking out *all* the items shall count as expressing a "statement" in this notation is therefore parallel to the question we were just dis-

cussing as to whether an utterance like "$N_5W_5S_5E_5$," which calls for no action, should count as an "instruction." It is like asking whether the words "Do anything you like" shall count as a command. We could choose to frame the regulations for giving dinner orders in such a way that the striking out of all the items was taken to be a *breach* of the regulations. Or, instead, we could also allow *all* the items to be struck out, though we would not then expect that such an order could be obeyed in the way that other orders are obeyed. (Of course the null order could here be construed as a command *not* to bring anything—but this is not an order-for-a-dinner.)

(3) The last example recalls the mathematician's use of an expression like "point at infinity" or his custom of describing a straight line, say, as a circle "of infinite radius." When we say "parallel lines meet at a point at infinity" it would have been equally correct, though less convenient, to say "parallel lines do not meet at all." To transform one of these statements into the other is to change the meaning of the term "point" (and so to change the language of which that word is part). The peculiar status of the points "at which parallel lines meet" is shown by the added words in the phrase "point at infinity." Such modes of expression are not introduced wilfully into mathematics, merely to mystify the layman. Mathematical fictions have a use; but they must be recognized for what they are, if senseless questions are not to be generated.

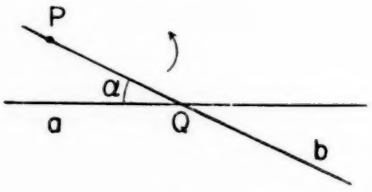

Fig. 1

One consideration that leads a mathematician to talk about "points at infinity" can be shown by the following example. Imagine in the above figure that the line *b* revolves anticlockwise

about the point P. As it does so, the angle between the lines a and b progressively diminishes, and the point of intersection, Q, moves progressively toward the right. By making the angle α sufficiently small, we can send Q as far as we like toward the right; and α can be made as small as we like. The *limiting position* is that in which the two lines have become parallel and the angle of inclination, α, has become zero. Now it is very tempting to add,

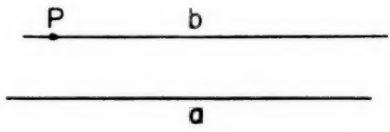

Fig. 2

as if it were a mathematical discovery, "and the point Q is then located infinitely far to the right." This addendum is, of course, nonsensical if "point" continues to be used in the old sense in which lines must *not* be parallel if they are to intersect in a point. The parallel lines simply do *not* meet and hence there is no point anywhere at which they meet. But it is in just such a situation that the mathematician often finds it useful to *extend* his terminology in such a way as to include the limiting case. Nothing prevents him from saying "The lines *do* meet—at a point at infinity"; for "meeting at a point at infinity" now means what "*not* meeting" meant before.

Similar considerations apply to the renaming of a straight line

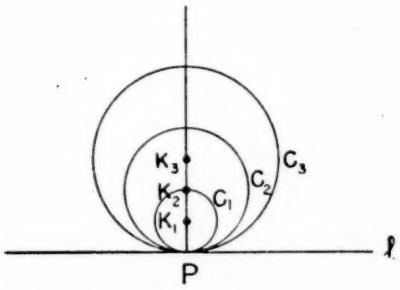

Fig. 3

as a "circle of infinite radius." We think now of a series of circles as shown in Figure 3, of constantly diminishing curvature, and all touching a given line l at the same point P. As we take larger and larger circles, C_1, C_2, C_3, . . ., C_n, . . ., the curvature of the circles diminishes without limit towards zero—and the centers of the circles, K_1, K_2, . . . K_n . . ., are located further and further from P. Sometimes the mathematician will say that the line l, which is the geometrical limit to which the circles converge, *is* a circle of "infinite radius" and "zero curvature." This choice of terminology allows him to speak of what were previously called "circles" and what were previously called "straight lines" in a *single* theory. Such unification is one of the main results achieved by the introduction of mathematical fictions.

When the limits of series (whether of geometrical figures or other mathematical entities) are described by applying to them the language previously reserved for *members* of the series, certain anomalies result. (For example the analytic equation of a straight line is of the first degree, while that of a circle is of the second degree.)

In general, the limit of a series of things all having a complex defining characteristic K, will *not* always have the property K: the limit of a series of intervals may be a *point*, the limit of series of rational numbers an irrational number, and so on. If the language is now so extended that the same term "K" is used henceforth to apply also to the *limit*, the difference between it and the terms of the series will reappear as an apparent difference in "properties." The limit will be regarded as a "peculiar kind of K" —or as "a K with peculiar properties." When the "properties" of the limit appear simpler than those of K's in general, it is sometimes called a "*degenerate* case." Thus we can regard a straight line as a degenerate case of a circle, or, again, zero can be regarded as a degenerate case of an integer. ("Degenerate" has the force of "abnormal," with an additional suggestion of regression to the relatively undifferentiated or more primitive. Thus zero follows special or abnormal rules for computation; again many arithmetical operations performed upon zero—doubling, squaring, and the like—merely lead back to zero again.)

IX

Some necessary statements can be regarded as limiting or degenerate cases of *contingent* statements. Consider, for instance, the four statements:

(a) Tom is home and Dick is home.
(b) Dick is home.
(c) Tom is home or Dick is home.
(d) Tom is home or Tom is not home.

Of these, (a) entails but is not entailed by (b); (b) entails but is not entailed by (c); and (c) entails but is not entailed by (d). That is to say, it is logically impossible for (a) to be true but (b) false, though not vice versa; and similarly for the other two pairs. This set of relationships is sometimes described by saying (a) is "logically stronger" than (b), (b) than (c), and (c) than (d); or, again, by asserting that each statement "says more" than the one next to it in the series. These modes of expression make it natural to say that (d) is the "logically weakest" of the four, and "says less" than the other three. Indeed, since there is no further statement that is entailed by (d) but does not itself entail (d), it is very natural to take the further step of asserting that (d) "says nothing at all." The same line of thought leads us to say that all statements of minimum strength "say the same," that is, nothing at all. As Wittgenstein puts it, "A tautology follows from all propositions: it says nothing"[6] and again "All propositions of logic say the same thing. That is, nothing."[7]

This way of regarding necessary statements clearly treats them as limiting or degenerate cases of statements proper (that is, contingent statements). Among the reasons that lead us to treat the sentence "Tom is at home or not at home" as expressive of a "statement" are (i) that the sentence is well formed and is clearly not "nonsense" in the pejorative sense of that term, (ii) it has cognitive uses, (iii) it can be inserted into a series of sentences expressing statements arranged according to "strength" or "logical priority." Such considerations incline us to say that necessary

[6] *Tractatus*, 5.142. [7] *Ibid.*, 5.43.

statements are statements in good standing. But there are balancing considerations that incline us to regard them rather as *limiting* cases of statements proper. For, seeing that necessary statements cannot fail to be true, we feel inclined to say they do not convey differential information and in this way resemble the limiting or empty commands that make *no* demands upon their recipients. From this point of view, the presence in our language of necessary statements is due to the benign redundancy of our notation.

A necessary statement is naturally regarded as "degenerate," inasmuch as its logical relationships manifest abnormal simplicity: every proposition entails a tautology—and all tautologies entail one another. The seeming paradox of such remarks as "all tautologies say the same, that is nothing" is another characteristic sign of a degenerate case. If we use the word "say" as a way of indicating the *information* conveyed by statements, we should say not "tautologies say nothing" but, rather, "it is absurd to ask whether tautologies say anything." The queerness of saying all tautologies have the same subject matter arises from our determination to overlook the differences between necessary statements and contingent statements by calling all of them "statements."

Yet I do not think the view of necessary statements as "tautologies," illuminating as it is, will cover all cases of what we would normally wish to call necessary statements. Of course, if we use "tautology" as a mere synonym for "necessary statement," as some writers do, it will necessarily follow that all necessary statements are tautologies; but, by the same token, this will itself then be an unilluminating tautology of the form *A is A*. It seems wiser to reserve the term "tautology" for a narrower usage, as referring to statements whose form is such that we can *prove* from definitions of the terms occurring that they are "self-cancelling."

Wittgenstein, who introduced the term "tautology" and is to that extent responsible for the term's wide currency, was thinking in the first instance of the necessary statements of the propositional calculus. There, the various logical operations ("**V**", "~", and the others) are defined by means of truth tables, so that the demon-

stration that a particular formula is a tautology requires the calculation, on the basis of these given definitions, of the truth value of the whole formula. When this is done and the result is found to be always "truth" it is intuitively plausible to say that the various logical operations cancel one another and therefore "say nothing." But this method of calculating truth values is not generally available, even in the artificially simplified calculi of mathematical logic. When the only method for showing a formula in the functional calculus is to derive it from accepted axioms, it becomes less illuminating to regard the theorems or the formulae as "empty tautologies."

The more general mode of description is in terms of what I have called "dummy assertions." For tautologies can be regarded as limiting cases or degenerate cases of statements, can be counted as statements in order to simplify the rules of inference. But not all necessary statements can profitably be regarded as tautologies, while they *can* all be regarded as dummy assertions.

X

I have now said all that I usefully can, at this stage, about the distinctive ways in which necessary statements are *used*. I have regarded them, mainly, as *surrogates* for rules, formulae resembling (and yet, also, not resembling) contingent statements, that serve as supplements for the purpose of simplifying the patterns of valid inference. And now we shall do well, I think, to take up the question of how, in this view, the assertion of necessary statements can be justified.

The circumstances in which I shall conceive the demand for a direct justification to have arisen are as follows. One person, A, has affirmed some necessary statement, say "Nobody can be in two places at the same time" and his hearer, B, is *genuinely* in doubt about its truth, and is seriously asking that reasons be supplied. (By stipulating that the doubt be "genuine" I mean to exclude cases when a philosophical doubt is raised, that is, when the protagonist is concerned with how *any* necessary statements could be defended, not with the defense of the particular necessary

statement under consideration. Cf. the difference between somebody who wants a reason for believing that he will see a bear in the woods and the skeptic who wants reasons for believing *anything* about what is not observed at the time of utterance.) The specification I have given requires that B, the doubter, shall understand the necessary statement that A has made, according to the normal English meaning that is customarily attached to the words that A has used. I am excluding a case when B fails to understand what A has said; the case in which A is using words in some way other than that of the standard English usage is discussed later.

Granted that B understands A's statement, the view I have been defending requires A to direct B's attention to the rule (of the English language) to which reference is made in the assertive content of the necessary statement in question. In other words, A must show B, if a direct justification of the necessary statement is to be provided, that there *is* a rule in English providing that a statement of the form "X is now at Y and Z," when Y and Z are different places and X is a person, shall have *no use*.

Now there are two ways in which this could be shown. The first mode of direct justification makes the evidence for the truth of the necessary statement consist in certain complex facts about the behavior of typical members of those who would be regarded as speaking correct English. In some cases the evidence will be hard to get and when found may point in opposite directions; if this is so, the alleged necessary statement really will be problematic. But I think this is not so in the case in question. I think we know that persons who speak standard English would recognize that it was wrong to say "X is at Y and also at the same time at the other place Z"; and that, however puzzling it may seem to us in our philosophical moments, we have the complex evidence needed; we do in fact know it. This way of gathering evidence about the relevant rule might be called external: it might conceivably be followed by somebody not himself a member of the English speech community, just as the character of the rules of chess might be empirically determined from the behavior of chess players by somebody who could not himself play chess.

But the questioner B, by hypothesis, does know how to speak English and is therefore more intimately acquainted with the

relevant rules than any external observer could be who himself had no command of the language. So A might most effectively ask B himself to reflect upon how *he* would use the phrase "*X* is at place *Y* and also at place *Z*." Such "reflection" might better be described as a rehearsal of usage. We ask B to describe the circumstances in which he, speaking undistorted English just as we do, would regard it as correct to apply the sentence. This might be called a search for internal justification. This seems to me the more important kind of justification and in some sense more primary.

The outcome of this process of reflection might take one of the following forms: (i) B, after thinking of all kinds of situations, decides it would never be correct to call any of them cases of "*X* being at place *Y* and also at place *Z* at one and the same time." He has then recognized that the relevant rule is in force and the necessary statement has received "direct" justification.

(ii) B describes some situations which he thinks it would be correct to describe in the manner indicated. The only question between A and B would then be whether B has changed or distorted standard English usage. But this is a trivial question for all except linguists. B is entitled, within reasonable limits of prudence and courtesy, to speak as he pleases; by indicating the situations when he would apply the formula in question he has made so far plain the rules governing *his* discourse.

(iii) B is unable to describe any situations in which he would apply the words in question but obstinately reiterates that all the same "a man *can* be in two places at once." This is now sheer dogmatism on his part; we cannot prevent him from uttering those words—nor would we be interested in physical coercion, anyhow—but we must say that he has given no sense to his utterance.

(iv) B recognizes the rule, viz., that "*X* is both at *Y* and at *Z* at the same time" shall not be used, but asks *why* the rule should be obeyed. Now it is no longer clear what he is requiring or what will satisfy him. We might, of course, talk about the practical benefits of speaking the same language as others but it may be

presumed that this is not what he wishes to hear. There is no other sense of "justification" that he has made clear to us.

The details of the above discussion could be exactly paralleled by considering what could reasonably be done to satisfy a doubt that a given statement really did represent a rule of some game. The peculiarity of the case in which somebody wants a necessary statement (and not the rule of some conventional pastime) to be justified is that the question does not arise except for somebody who already speaks the language, that is, is already bound by and recognizes the relevant rules. And this is the source of the feeling we have that what is said in a necessary statement "could not be otherwise." To give anybody a direct justification of an unequivocally necessary statement is simply to put him in a position to recognize more clearly and more forcefully the rules to which in some sense he is already committed. But I am not saying that the necessity of the necessary statement has anything to do, logically speaking, with the feelings of those who use it. Its necessity, according to my argument, consists in its relation to an associated rule of language. And this account, right or wrong, does not offer any psychological criterion of necessity.

XI

We can now see more clearly what the function of "intuition" and the *Gedankenexperiment* really are in establishing necessary statements. Let us consider the simplest possible case, since this will show what is involved just as clearly as a more complicated instance. It is not inconceivable for somebody to have been using the words "violet" and "lavender" to refer to the same shades of color, without ever having reflected upon the connections between the meanings of these words. Upon being asked for the first time whether violet and lavender are the same he "imagines" something violet and something "lavender" and, as he says, "sees" for the first time that both are the same color.

Now this *Gedankenexperiment* can be even more effectively performed upon actual objects than upon images. Let us suppose, therefore, that our subject has been provided with a large number

of uniformly tinted pieces of cardboard. First he hunts through the pile until he finds a piece, say *A*, which he recognizes as "violet." Next, he goes through the pile again until he finds a piece, say *B*, that he recognizes as "lavender." The crucial step, for our purpose, is that in which by "looking" at *A* and *B* he "sees" violet and lavender are "the same." If *A* and *B*, the specimens he actually chose, are the same color, this is an empirical fact, just as much so as is the fact, say, that they are both of the same temperature. In order to derive the necessary statement, we need as premises statements about the *colors*, not merely about the pieces of cardboard that have been inspected. The premises might be:

(i) This color is violet

(ii) This same color is lavender

from which, of course, it does follow that lavender and violet are the same. But the assertive content of (i) is not a fact about anything except linguistic usage. On my view the person who affirms (i) and (ii) is simply saying—so far as the assertive content of (i) and (ii) goes—" 'violet' and 'lavender' are both used correctly in this kind of situation." The effect of the color cards is to remind the speaker forcefully that he does use "violet" and "lavender" interchangeably. This is what he "sees" and not some alleged fact concerning the identity of a color with itself. There *is* no fact or truth to the effect that a color is identical with itself, and so there is nothing of this sort for even the most penetrating examiner to "see."

Now the assertion that two words are synonyms cannot be conclusively verified by a single instance of identity of application. The man who looks at the two colored pieces of cardboard must assume that he has representative instances before him, and it is conceivable that he might make a mistake about this. There might be other specimens that he would call violet and refuse to call lavender, or others that he would call lavender and refuse to call violet. Of course the affirmation of the necessary statement "violet is lavender" itself renders it unlikely that this will occur; the affirmation of a problematic necessary statement has a tendency to make that statement true.

I will agree that the "flash of insight" that sometimes crowns the performance of a *Gedankenexperiment* may have considerable heuristic value; it may indeed be an excellent index of the truth of the necessary statement concerned (though it may also be deceptive, as countless instances attest). My central point is that the occurrence of such flashes of insight is not logically relevant to the truth of the necessary statement, being neither necessary nor sufficient for the truth of that necessary statement. If a man *does* follow the rule that "*A*" and "*B*" are interchangeable, the statement "*A* is *B*" when pronounced by him *is* necessary, whether or not he has the power to practice what has been sometimes called "intuitive induction." Conversely, if he has a flash of vision in which he "sees" that "*A* and *B* are the same," but in his practice does *not* use the two terms as synonyms, then the statement "*A* is *B*" is *not* a necessary truth when he utters it, no matter how much he protests that he has intuitive certainty to the contrary.

XII

I hope the account I have given has managed to avoid some of the weaknesses of "conventionalism." The essential point of difference is that the view here presented does not regard the necessary statements themselves as "arbitrary." On the contrary, for the man who has already accepted the relevant linguistic rule (and only such a man can entertain the necessary statement) the truth of the necessary statement is already determined and not within the speaker's power to change. That many instances arise in which the associated rule is not unambiguously in force, I must certainly agree; and in such cases it is indeed for the speaker to choose the rules by which, if he so wishes, he will henceforward be governed. But these are not the cases I have regarded as typical for the certification of necessary statements. In proportion as the rules are not yet firmly established in the language there can, indeed, be no question of the "truth" of the corresponding necessary statements—nor, for that matter, will there be any necessary statements in any strict sense of "necessary."

To those who still desire a "ground" or "reason" for the

linguistic rules themselves, I can offer no other than pragmatic considerations of convenience or usefulness. And if anybody objects that this leaves necessary statements without any justification at all "in the last analysis," I shall have to retort that they have been granted all the justification that can be reasonably demanded.

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PICTURE SPACE*

PEOPLE tend to think that though there are quite different sorts of outlook on things—religious, aesthetic, scientific, for example—yet the space in which their objects appear is, objectively speaking, a constant, or one and the same space in so far as the objects can truly be said to be in space at all. They are either in this space or they are subjective or mere appearances. Of course, the sophistication of the current physics of relativity makes it proper to speak of spaces in the plural, but still, where the space conceptions are not those of pure geometry, they are all said to have "physical meaning," and the spaces thus determined are poor habitats for anything but physical objects and properties, to be seen as such—or else.

In this sense, there is still, if not one space, then one kind of spaces, objectively speaking. Subjectively or metaphorically speaking, there is, if you like, also perceptual space or the space of sensations and images. Either the relation of such spaces and their objects to physical space is said to be embarrassing, or the embarrassment is dispelled by saying that the problem is unreal, since, literally speaking, there are no such renegade spaces.

It is this way of thinking and talking about space which I want to explore and explode. The current linguistic method of treating the problem has its merits, showing that the logic of the terms "space" and "physical" does not require one to think that space can be the habitat only of dead or mechanical things. But it does not show enough. The Oxford philosophers tend on the whole to continue talking as if whatever is there in space is primarily, or in the clear-cut case, a physical object, at which base we learn the primary meaning of terms, whatever stylistic graces other uses of them may have, not accountable for by the grammar of literal and physical thinking. The recent essay by P. Alexander, "Complementary Description,"[1] illustrates this nicely, where

* Read at the Annual Meeting of the American Society for Aesthetics in Washington, D.C., Oct. 25-27, 1957.

[1] *Mind*, LXV (1956), 145-163.

physical things and properties in physical space are taken as the paradigm and literal base of sense making. The rest remains in somewhat embarrassing relation to this and jeopardized by its alleged priority. In short, we are said really to see only physical objects and properties, learn the meaning of basic terms in this primary rapport, then proceed at our own risk to metaphor and the other imaginative secondary uses of expressions, in the non-literal constructions we place on things. Thus, art and even psychology start at home base with strikes against them, their data being strictly in the nonaesthetic area controlled by the view of things as really physical, and by the logic that becomes explicit in natural science. Some of the Oxford philosophers put the case with a less obvious bias in favor of physicalism, even with some positive effort to get beyond it to a genuinely neutral position about the nature of things and of an objective logic. It is in this direction that we need to make more progress.

The question may come up, why care to be an objectivist in art? The answer is that illusionism, even just subjectivism, in art has its nemesis. In the end the hearty subjectivist and illusionist generally espouse a sort of metaphysics to the effect that reality is irrational, hence the greater value of art-as-illusion, as over against the superficial objectivity of science. So the concept of the really real is brought in by the back door after being thrown out the front. I am thinking of the surrealists especially as a good example (Herbert Read). But other and better reasons for maintaining an objective theory of art are lurking both in the situation called aesthetic, and in the very procedure of science itself, without however having to make science look like art either in its elementary or its advanced and finished phases. After all, we do occasionally think and speak of things as "aesthetics objects." It is this common recognition that should be justified in a straightforward way, together with the acknowledgment of the special logic of the expression of things in this manifestation.

The point can be sharpened, and made nicely, by concentrating on the visual art of painting, where light and color are the main thing. There the question of picture space, akin to that of color space in general, is prominent. The right way out of problems in this special connection will suggest the right sort of answers to

questions in other aesthetic cases which here will not be brought into focus.

Whether colors pure and simple and in general are extended at all in any space has long been a moot question, with support for both the affirmative and the negative side. Explaining this division is worth the trouble. Both sides are right on some count, wrong on another. On the side of the view that colors are not extended is the Berkeleyan type of argument that space, being a construction in some sense, is not simply experienced or given, whereas colors are; therefore colors as data are not extended in space, despite the spatial appearances that accrue to them discursively. And colors are always data, if anything. The opposition argues that if color is not really extended, nothing really is, a consequence it finds intolerable, though accepted by Berkeleyan mentalists. This affirmative side holds, with Kant and C. I. Lewis, that space is indeed a constitutive form of experience, not a simple datum like a sensation, but that there is a sense of color predicates which construes colors as objective properties. Here again, unfortunately, the extended objects thus qualified by color are thought of as physical objects only. Color as *aesthetic* quality tends in this conception to collapse into subjective sensation, but in a way that leaves the theorist uneasy. Lewis brings in the notion of inherent value in the effort to abate the malaise.

Let us pick out now what concerns us most in this dispute. What is recognized by both sides in this issue is that there is a determinable field of determinable things, and that these are not spatial except for certain determinations or categorial constructions, under which the field appears as determinate spaces containing things as determinate sorts of objects with the appropriate properties. A Berkeleyan will reject all space-determinations including the physical-space sort as mere appearances, while his opposition will tend to recognize as valid only the latter special kind of spatialization. The Berkeleyan prefers the immediacies of secondary and tertiary qualities which strictly are, he says, in no space at all, while the space proponent supports the physical context or space of primary qualities, in spite of the dislocation this involves for the aesthetic.

We can see what is wrong with both sides falling into this sort

of opposition if we get back into the aboriginal and neutral milieu which I have called the determinable field of determinable things, the context prior to any *special* space construction, and talk our way back into the special spaces of things as determinate kinds of objects. The important thing to notice is that an expression such as "red thing here-now" makes no sense either for science or for art without certain exclusive restrictions involving a special way of looking at the thing and of expressing it. Phenomenalism is wrong in supposing that it does, without reservations, make sense as an elementary expression basic to science, a mistake that Carnap eventually realized. The "here" and the "now" and the "red" must be taken from scratch as having physical meaning or referring to factors determined by observation, if the expression is to serve as evidence for scientific generalizations and hypotheses (at place p at time t, a color resembling a standard r). And this will presuppose a category of a determinate space which defines "observable" and requires an exclusive special kind of awareness distinct from, say, sensation or imagination. You may have noticed that the terms "observation" and "observable" have occasioned some embarrassment in the empiricist camp, since these key terms themselves do not behave like empirical terms subject to that sort of definition. The fact of the matter is that they are category terms, presupposed by the scientific enterprise, and necessary to its delimination and definition as the special and exclusive thing that it is, both as a way of looking at things or a form of attention, and with regard to its logic of statement making. To "observe" something is an achievement under a categorial requirement—Ryle properly calls "observe" an achievement word—without which science would not be grounded. A simple, unrestricted noticing of things does not provide science with its special foundation.

That an exclusion functions in the observational case is my over-all point. But care must be taken about its statement. This exclusive sort of attention is something like the awareness of Jastrow's duck-rabbit picture as, say, a rabbit. What is excluded is the duck aspect of the picture. And this does not exclude a part of the picture or drawing. Either we say that the "whole picture" is seen as a rabbit or we say that the whole-part distinction does

not significantly apply to the case of aspects, in Wittgenstein's use of the term, which is my use here. But what I am suggesting is much more fundamental. I am suggesting that things may be seen as physical objects or as aesthetic objects, and that these are categorial aspects of things appearing under different forms of attention; and that the whole aboriginal nature of things, simply noticed in the neutral field, may appear and be expressed under one of these aspects or the other; and that neither aspect has ontological priority over the other but are special ways the determinable things are determined for kinds of experience and expression. This is what I have been meaning by the notion of the special spaces (aspects) in which things appear as sorts of objects under forms of attention that determine the field as a physical or an aesthetic region. The point is a little like Spinoza's about formal attributes each of which expresses the whole essence of substance as the ultimate reality, and whose special logic does not allow it to be confused with the other attributes.

Prior to these special determinations of the determinable field, ordinary experience is the occasion for things to appear ambivalently, suggesting or portending both physical and aesthetic formulations as achievable by the use of abstractive techniques. There is an artless attending to things under both aspects at once in this common non-special case, in which presentation neither aspect is clearly prehended or actualized, each tending to denature the other. Ordinary language expresses this situation, with a comfortable accommodation for both potentialities. When the looking and the language become technical—and both these developments occur together—there is the dawning of an aspect. One sees things as physical objects in physical space, and this is to "observe" them; or one sees them as aesthetic objects in their proper space, which mode of awareness I shall call "basic imagination." I say "basic," because it is a mode of perception. The notion of "subjective" imagination is another concept not yet imported into this scheme.

I must now bring all of this down to earth by illustrations, showing how the field of things is determined specifically as aesthetic, or describing the space of a painting in which determinable things in the field are seen as determinate aesthetic

objects in a nonphysical space. Our approach so far has been a sort of mapping of the whole terrain, an aesthetic aspect of which—picture space—is our main interest.

We need first to illustrate the detection, by the painter's eye, of the aesthetic aspect in the field not yet determinately spatialized. Suppose it is Kokoschka looking at an Alpine landscape. Remember that he knows the language of the technique of painting. Without this he could not see with the painter's eye; he would lack the visual intelligence necessary to the sort of perception that can exclude the physical aspect in favor of the aesthetic. The better his grasp of the language or medium of expression, the more acute and exclusive the looking. These develop together, both beginning in an inconspicuous and ending in a maximum achievement. This kind of looking, called basic imagination, is also an achievement under a categorial condition, as is observation. What is the difference between observing the scene and basically imagining it? Kokoschka is engaged in the latter, which means that he is not incipiently aware of end-to-end coincidences of rigid units of measure structuring the field and thus determining distance intervals and volumes. He sees the house as a color expanse whose outline extends on one side into that of one of the mountains, making them conterminous. And he sees color tones in house and mountains that play all of them into the same plane as a flat texture, with just enough hollows and protuberances to make the plane interesting. But this aspect initially, or in the state of nature, is not exhibited as determinately as it might, owing to the portent there of other aspects which tends to undo the object of his basic imagination. So he secures it against such intrusion by painting the picture and framing it; then the landscape can much more readily be seen as he saw it. Thus does he show us an aspect. We will see it only if we too have at least some elementary grasp of the logic of that medium of expression. And he can help us learn it by talking to us about his painting, either while he paints or afterwards, showing us how we are to look at it, or, best of all, showing us how to paint.

Now we get to the crucial point. When we see the house and mountains as flat in the painting, we are not at all seeing the flatness of the pigmented surface of the canvas as a physical

object. It is a complete mistake to suppose that what recent painters say about their feel for pigment as such, and their loving recognition that what they are brush stroking or painting is a canvas, means that they want you to see the work of art, the painting, as a flat physical object. The painting is a house-and-mountains picture, and we are to see this as house-and-mountains in pretty much the same plane of light and color. Flatness here is a concept under the category of aesthetic space. And in Kokoschka's particular picture, these space-properties are fixed by a brush-stroke technique and the juxtaposition of various pigments in a way that makes the subject matter determinate (flat) in the space of his picture, while inducing the relevant way of looking at it. This technique as a whole, this idiom in brush stroke and pigment, is different in kind from the method or logic of determining the space properties of things as physical objects, subject to observation in a reference frame of fixed coordinates. So the kind of space, determining the way things are to appear in it, is categorially different. The movement, energy, rest, depth, surface *of a thing* will be exhibited in this space as "actually" as in the physical, but they will be of a different order, and the terms of their expression under this aspect will have another use or meaning, a different logic, which I shall comment on at the end. I suggest that we call things "real" as they simply appear in the neutral field prior to any special view and articulation of them; and that we say they "actually" are what they are in a special view and expression of them—which actualizes a potentiality for special treatment. By this stipulation, real things, simply as such, cannot serve as data for any special discipline, and the aesthetic mode of experience and expression is ontologically on a par with the physical. Both are special formulations of a real subject matter that lends itself as readily and as completely to the one as to the other. Also, this is the place to notice that there are peoples or cultures to which the dominant or natural outlook is the aesthetic from the ground up, as in the case of, say, the Japanese, who commonly need as much assistance to see things as physical objects as *we* commonly need help to be introduced to the basically aesthetic view, in our scientific ethos.

In my concluding remarks I shall concentrate on the notion of

seeing something as something, since this, in its aesthetic application, will throw more light on picture space and on the general problem of representation in art. There are three cases to be distinguished: (1) seeing that something *is* something; (2) seeing that something is *like* something; (3) seeing something *as* something. Let us examine the relevance of each of these to aesthetic experience and expression.

If something is to be aesthetically experienced, there must be some awareness that the thing as it simply is is distinct from that thing as an aesthetic object. Where the thing is a painting of a man, for instance, one readily sees that the work of art is not a man; else one is deceived and the experience is not aesthetic. This can be generalized even for things in the state of nature. To see a real man in a way that suggests an aesthetic formulation, without distinguishing the man in this special view of him from the real man or the man simpliciter, is either to love or to hate him, and the experience is not aesthetic. The artist is keenly aware of this and so will delight in making the fact clear in his painting by leaving traces of the brush on pigment, and the like, thus driving home the fact that this, the work of art, is not a man. All this serves to show that in the aesthetic case we are not to see that something (the art work) is something (the original if any). Nor are we to see simply that the painting is a painting.

Neither will it do, in the aesthetic case, simply to see that something is like something, though occasionally resemblance will be a necessary condition of the aesthetic seeing. To see that the painting is like a man in some respects is not sufficient. Such a fact is noticeable by many people who have little aesthetic grasp but who delight in the recognition of a likeness. So again the artist is annoyed at the tendency to seek and be satisfied by the detection of resemblances. In this sense he is against representation in art. It obstructs the aesthetic view, since seeing that something is like something can occur in the neutral, non-special way, and even in physical space by observation of things as similar physical objects. Moreover, what is enjoyed in such experience of a likeness is the sense of being brought to the threshold of believing that something is something, a case of near-illusion without actual deception. The artist will, therefore, as he comes into his

own, make a point of painting a picture very visibly unlike anything, or of presenting the subject in a form sufficiently unlike the original from the point of view either of an observer or of a non-special onlooker, thus achieving the triumph of exhibiting the aspect he originally had in view without using an obvious likeness. So genuine art naturally seems cultish, or subjective or emotive, to those who rely primarily on resemblance in art.

What the artist seeks to induce is a seeing of something *as* something, and his painting is the expressive medium by which he achieves this. In the work of art, we see the aspect he has in view, in his sort of experience and thought of things. The distinction between the physical and aesthetic aspect can be made more clearly by scrutinizing not simply the case of a thing in the state of nature under the two different aspects, where the contrast is not sharp, but an artificial thing such as our painting. What I mean by seeing something as physical *or* as aesthetic is illustrated conspicuously in the work of art *(objet d'art)*. To see the painting as physical is to see the flat surface of the canvas covered with pigment and that it is approximately 24 by 32 inches, and so forth. Even the colors, seen this way, will have the sort of spread and context that determines them as observable properties relative to standards. And one does not simply see them this way. This aspect becomes salient only upon a special way of looking involving an incipient comparison with standards, together with some awareness of the logic relevant to its mode of expression.

But as the aesthetic aspect dawns on the situation the case is different. Not at all because we lose sight of the pigment and begin, so to speak, to look through it at an image as in a mirror. Certainly not this. Rather, a careful, sustained looking at the painting is a necessary condition for the dawning of the aesthetic aspect. This does not involve seeing the constellated pigments as physically flat, but rather it involves taking another sort of good look at them, which then is to see them as the house-and-mountains, or alternatively to see house-and-mountains depicted as flat in the space of Kokoschka's picture. Such seeing may presuppose some point of resemblance between work of art and original, but it remains essentially different from seeing that likeness. Of course, the pigments might have been deployed to

exhibit an aspect of great depth and contrasting volumes in different planes, which then would present another aspect of the house-and-mountains. Thus the painter's eye may also see them as not flat. One remembers van Gogh's complaint to Gauguin that things in his picture-space are so flat, and Gauguin's vehement rejoinder, "That's the way I see them—flat!"

These remarks of mine show that I have been using "aspect" in two senses, one fundamental or categorial, the other what I shall call modal or secondary. I must now distinguish these. The "physical" and "aesthetic" aspects are categorial, and the terms are category-terms. Such are presupposed by or necessary to the definition of the scientific and aesthetic enterprises in general. Such terms as "observation" and "basic imagination" accompany them as also having categorial uses. In this sense they stand for achievements and are achievement words. Moreover, categories in this sense are not "mental" or "imposed on things by the mind." They are structural aspect-like attributes of things.

But given one of these aspects as fundamental and normative for procedures under it, determining the kind of experience and expression, then there are the secondary or "modal" ways of looking at things and expressing them under that category, and this brings "aspects" to light in the secondary sense of the term. The artist may see things as solid or as flat, both under the aesthetic form or category. This is like seeing the duck-rabbit picture as a rabbit or a duck. But even in this sort of case, if the look is a good one, the aspect will be "there" to be perceived under the appropriate category and featured in the medium of articulation.

Now for the final brief word, promised above, about the logic of aesthetic expression of aspects. It is somewhat like the logic of sensation and image terms as described by Wittgenstein. In that case no significant question of justification of the expression can arise for the user himself, but only for others who are to accept it. And the criterion of acceptability for them, according to Wittgenstein, was the user's say-so, in the context including his known truthfulness and general ability at the language game he is playing. There is no call for observing the "content" of his expression as a check on its truthfulness, where sensations and images are the subject matter.

These are, mutatis mutandis, the rules governing the acceptability of expression of aesthetic aspects, in what I have called basic imagination. But there is a difference. We expect, eventually at least, to be able ourselves basically to imagine what the painter does and in that sense to see it in his work of art. And even the artist had better raise some question of justification of his own expression, not with respect to its truthfulness—that he does see this or that aspect—but with respect to the importance of the aspect he has in view for his own aesthetic purpose, which he will in some measure share with others in that pigment language game. He must himself employ his medium in a way that does justice to the aspect he sees, and take some care to feature the aspects that are more expressive of the nature of things in picture space. And to achieve this will be to exhibit things in a region in which seeing them will, in effect, become a visual way of feeling them, with emotional overtones that are quite at home in that imaginative space as qualities of the subject matter. All this is possible because things are not initially or "really" in a region that precludes picture space.

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DISCUSSION

THE LOGIC OF COLOR WORDS

AN ALTERCATION between Hilary Putnam and Arthur Pap has recently appeared in the *Philosophical Review* concerning the "good, old-fashioned" analyticity of color incompatibility statements such as "Nothing can be both red and green all over at once." Starting with the primitive relation, "x is indistinguishable in color from y," ["I(x,y)"], Putnam[1] constructs the reflexive, symmetric, and transitive relation, "x is exactly the same color as y" ["E(x,y)"] by the stipulative definition,

$$\mathrm{E}(x,y) =_{\mathrm{def}} (z)\ [\mathrm{I}(z,x) \equiv \mathrm{I}(z,y)] \qquad \mathrm{D}_1.$$

From D_1 and the further definitions of "F is a color" ["Col (F)"]

$$\mathrm{Col}\ (F) =_{\mathrm{def}} (\exists y)\ (x)\ [F(x) \equiv \mathrm{E}(x,y)] \qquad \mathrm{D}_2,$$

he is able to deduce, by straightforward logical procedures, that nothing is two different shades of color at once—that is, that

$$(F)\ (G)\ (x)\ (\mathrm{Col}\ [F] \cdot \mathrm{Col}\ [G] \cdot [F \neq G] \supset \sim [F(x) \cdot G(x)]) \quad \mathrm{T}.$$

Pap,[2] in turn, while accepting the bulk of Putnam's analysis, points to the need for certain qualifications (principally, in regard to whether "Red is a color" may be treated as analytic in the strict sense) to which Putnam[3] acquiesces.

Now, it is no doubt presumptuous of me to intrude into a private argument which, moreover, appears resolved to the mutual satisfaction of both parties. Yet I feel that Putnam's analysis is both too ingenious and too seductively misleading to be put aside without further exploration. I shall attempt to make the following points: I. Putnam's definition of "E(x,y)" [D_1] does not adequately analyze our intuitive notion of "x is exactly the same color as y," and indeed falls short of his own

[1] "Reds, Greens, and Logical Analysis," *Philosophical Review*, LXV (1956), 206-217.

[2] "Once More: Colors and the Synthetic A Priori," *Philosophical Review*, LXVI (1957), 94-99.

[3] "Red and Green All Over Again: A Rejoinder to Arthur Pap," *Philosophical Review*, LXVI (1957), 100-103.

criteria for such an explication. II. Putnam's analysis of color predicates cannot satisfactorily be extended to the general case of multiple coloration. III. The claim that nothing can be two different colors all over at the same time is true only when sufficiently qualified in a way that *does* make it good, old-fashioned analytic, and in demonstrating this we seem forced to recognize that there are two basically different ways in which color predicates can be applied to objects.

I

As Putnam has properly assumed, the relational predicate, "x is exactly the same color as y" is an important color concept which, given a satisfactory analysis, might be expected to perform heroically in the explication of further color concepts. We may also agree that, while indeed difficult to pin down, "x is exactly the same color as y" does seem to be a stronger, transitive version of "x is indistinguishable in color from y," which is intuitively reflexive and symmetric, but demonstratively not transitive. Therefore we may accept Putnam's stipulation that any construction acceptable as an analysis of "$E(x,y)$" must (a) be reflexive,[4] symmetric, and transitive; and (b) must analytically (in either the strict or the broad sense) entail "$I(x,y)$." For otherwise we should be able to imagine a world in which "$(x)(y)[E(x,y) \supset I(x,y)]$" did not obtain, so that "$E(x,y)$" could not be said to be a stronger version of "$I(x,y)$" in any meaningful sense.

But one begins to wonder whether the "meaning content" of "$E(x,y)$," as explicated by D_1, really does include that of "$I(x,y)$" when one observes that the reflexivity, symmetry, and transitivity of "$E(x,y)$" follow analytically from D_1, irrespective of the reflexivity and symmetry of "$I(x,y)$." It does not seem unfair to be suspicious of so much received at so little expense, and this suspicion becomes strengthened upon realization that contrary to stipulation (b), "$E(x,y)$" does *not* entail "$I(x,y)$," but only "$I(x,x) \equiv I(x,y)$." To be sure, we can

[4] Actually, the reflexivity of "$I(x,y)$" is not as intuitive as it might appear, for the claim, e.g., that a tone is indistinguishable in color from itself should make one pause for thought. However, it is even stranger to assert that a tone is *not* indistinguishable in color from itself, which is our only alternative unless we regard the expression as meaningless. While sentences which, though syntactically well formed, are intuitively distressing are not infrequently written off as meaningless, such arbitrary tactics find me wholly unresponsive. Hence I, at least, am willing to admit that color indistinguishability is a reflexive relation, whether applied to colored objects or not.

derive "$I(x,y)$" from "$E(x,y)$" and "$I(x,x)$," while "$(x)[I(x,x)]$" may be taken as a meaning postulate, but the fact that "$I(x,x)$" has to be adduced in *addition* to the analysis of "$E(x,y)$" for the derivation of "$I(x,y)$" shows that the material implication of "$I(x,y)$" by "$E(x,y)$," defined by D_1, is formally contingent, not analytic. Nor does it seem correct to say that "$I(x,y)$" can be obtained as a component in a meaning analysis of "$E(x,y)$," for this is to imply that a statement of form "$(x)[\Phi(x) \equiv \Psi(x)]$" has "$\Phi(a)$" as part of its meaning content for some individual, *a*.

The fact that "$E(x,y)$," defined as in D_1, is actually *weaker* than "$I(x,y)$" in meaning content accounts, I think, for the "paradox" which Pap constructs from D_1 and D_2. Pap[5] shows that if "red"—referring to a specific shade of red, say red_6—be defined as the color of some red object, *a*, by the definition, "$Red_6\ (x) =_{def} E(x,a)$" then "$Red_6\ (a)$" becomes an analytic truth in the strict sense, and the "formal definitions therefore warrant an 'ontological leap' to the existence . . . of red . . . things." Apparently accepting the adequacy of D_1, Pap goes on to argue that such a "startling" consequence challenges the presumption that color predicates can be defined in this way. Putnam,[6] in turn, concedes that this is indeed a deplorable outcome and retreats to interpreting specific color names as primitive terms. Now I shall argue later (in II) that D_2, which neither Pap nor Putnam wishes to abandon, does in fact commit us to defining specific color predicates in terms of color-identities with prototypical particulars. But such a definition in itself, even though it does involve us in an "ontological leap" of a sort, does so in a very inoffensive way, not at all peculiar to the present situation. As a preliminary, note that we are now referring to a specific shade of red, rather than to a generic grouping of the various reds. Therefore, Pap's argument that we abstract color qualities from *several* particulars may be irrelevant here, for the color terms of ordinary language seem to refer to classes of specific shades, whereas it is not at all implausible that we learn a specific shade by ostensive definition based on a single particular. And if by "$Red_6\ (x)$" we really *do* mean "*x* is exactly the same color as *a*," then "$Red_6\ (a)$" has exactly the same logical status as does "The standard meter bar in Paris is exactly one meter long." To be sure, neither of these, when "being the same color as" or "having the same length as" is taken as primitive, or is constructed from definite descriptions in which "color" and "length" are primitive, is strictly speaking analytic

[5] *Op. cit.*, p. 96.

[6] "Reds and Greens All Over Again," *op. cit.*, p. 102.

in the narrow sense (though, to borrow Putnam's phrase, they certainly *feel* analytic). Thus when "$E(x,y)$" is taken as primitive, deduction of "$\text{Red}_6\ (a)$" requires the meaning postulate that "$E(x,y)$" is reflexive. But here is not where the "ontological leap" contained in "$\text{Red}_6\ (a)$" lies, for "$(x)[E(x,x)]$" involves no ontological commitments. Rather, the damage is done by admission of "a" as a proper name or description [compare the deduction of "$(\exists x)\ (x = a)$" from "$(x)\ (x = x)$"].[7] So it is not the ontological commitments of "$\text{Red}_6\ (a)$," when "$\text{Red}_6\ (x)$" is defined as "$E(x,a)$," that should bother us, but rather its stark, uncompromising tautologicality under D_1. To say that a is red_6 in color, or that the standard meter bar is one meter long, is surely to say *something*, if only no more than what is asserted by instantiation of a meaning rule. But if "$\text{Red}_6\ (a)$" is reduced by D_1 to its ultimate definiens, we arrive at "$(x)[I(x,a) \equiv I(x,a)]$," which says nothing factual whatsoever. Thus D_1 must be inadequate as an analysis of "x is exactly the same color as y."

Now the objection raised so far to D_1 can be met very simply. We need but conjoin "$I(x,y) \cdot I(y,x)$" to the definiens in D_1:

$$E(x,y) =_{\text{def}} I(x,y) \cdot I(y,x) \cdot (z)[I(z,x) \equiv I(z,y)] \qquad D'_1,$$

which is analytically symmetric and transitive, and hence suffices to prove theorem T. Note that "$\text{Red}_6\ (x) =_{\text{def}} E(x,a)$" no longer gives "Red (a)" as narrowly analytic, for under D'_1, "Red (a)" entails "$I(a,a)$," and the latter can be obtained only from a formally contingent meaning rule about the reflexivity of the primitive, "$I(x,y)$." It is very dubious, however, whether *any* expression in which "$I(x,y)$" occurs is acceptable as an explication of "$E(x,y)$." For "$I(x,y)$" is based upon a triadic (or higher) relation involving two colored objects *and a discriminator*, and the logical complexities into which this gets us surely need not be spelled out here. Nor can we argue that use of "$I(x,y)$" gives a phenomenalistic reduction to "$E(x,y)$," for "x is indistinguish*able* from y" is a dispositional predicate.

II

But let us press onward, for more exciting game is afoot. Suppose that D_1 *is* acceptable as a definition of "$E(x,y)$," or that "$E(x,y)$" is

[7] Even admission of "$\text{Red}_6\ (x)$" as a primitive term involves us in an ontological problem. For then, from "$(\Phi)\ (\Phi = \Phi)$," we can prove "$(\exists\Phi)\ (\Phi = \text{Red}_6)$"—i.e., that there exists something which is identical with red_6-ness. But if red_6-ness has no exemplification, its "existence" is odd indeed.

simply taken as a primitively stronger version of "$I(x,y)$." We may now inquire whether D_2 is acceptable as an analysis of "F is a color." It will be noticed that D_2 is actually two stipulations neatly condensed into one: (a) it determines when a quality, F, may be called a "color" quality, and (b) it describes the conditions under which a given specific shade of color may be ascribed to a given object. I shall now argue that D_2 is inadequate on both counts. Putnam has shrewdly restricted his analysis to cases of homogeneous coloration "for the sake of simplicity." But examination of D_2 with its contextual restrictions made explicit demonstrates its inapplicability to the general case.

What can we say about the color of a checkerboard? Well, we would be unhappy if we were forced to deny that it is red *and* black—not just red-and-black in the way that we might say a grey object is at the intersection of the blacks and the whites, but two different *specific* shades. Yet as it stands, D_2 and the transitivity of "$E(x,y)$" ineluctably bar such a dual color ascription. Since Putnam reassures us that his analysis is restricted to uniformly colored objects, D_2 stands revealed as an ellipsis in which lies hidden the property of being uniformly colored. Let "x is uniformly colored" be abbreviated "$U(x)$." Then unpacking D_2 appears to give

$$\text{Col}\ (F) =_{\text{def}} (\exists y)(x)[U(y) \cdot (U[x] \supset [F(x) \equiv E(x,y)])] \qquad D'_2,$$

which leaves open the application of color predicates to multicolored objects.

But D'_2 will not do at all as a definition of "F is a color." To see why, let us first examine D_2 more closely. Pap has argued[8] convincingly that the biconditional in D_2 must be interpreted as *intensional* rather than material equivalence. That is, D_2 admits a predicate, "F," as a color name if and only if there is some object, y, for which "$F(x)$" is intensionally equivalent to "$E(x,y)$." But since the meaning of "$E(x,y)$" has already been stipulated in terms of "$I(x,y)$," which is primitive, then either "$F(x)$" must be *defined* in terms of "$E(x,y)$," or of "$I(x,y)$," or we have smuggled meaning rules (or worse) into D_2 as a component of the definiens, thereby obtaining the strict analyticity of theorem T only by building the synthetic a priori into the very concept of "color." (For this reason I contended above that Putnam appears committed to *defining* colors as color-identities with particulars.) At any rate, whether we insist that colors be defined in this way or allow a broader kind of intensional equivalence, D_1 would seem to

[8] *Op. cit.*, p. 94.

state that to be a color predicate is to *mean* being exactly the same color as some specified object.[9]

But the picture changes when the contextual restrictions of D_2 are made explicit in D'_2. For while "U(y)" merely limits colors to definition in terms of uniformly colored objects, "U(x)" says that for any object x, the (broad or strict) analytic equivalence of "$F(x)$" and "E(x,y)" is *contingent* upon x's being uniformly colored. That is, D'_2 (which is D_2 spelled out) admits as a color predicate *any* F, no matter what its *total* meaning content, which is constructed in such a way that for some uniformly colored object y, "$F(x) \equiv \mathrm{E}(x,y)$" may be logically deduced from "U(x)." But this admits predicates such as "Either x is exactly the same color as a, or x is a multi-colored baboon" which qualifies as a color predicate under D'_2 so long as a is uniformly colored. Hence D'_2 cannot itself be regarded as a *definition* of "F is a color," at least if "Col (F)" is to retain any pretense at analyzing our intuitive notion, and must at best be replaced by

$$\mathrm{Col}\ (F) \supset (\exists y)(x)[\mathrm{U}(y) \cdot (\mathrm{U}[x] \supset [F(x) \equiv \mathrm{E}(x,y)])] \qquad \mathrm{P}_1.$$

What, now, is the status of P_1? It is still adequate to prove theorem T for uniformly colored objects, but unless we are actually able to construct a satisfactory definition for "Col (F)" which entails P_1, the latter simply stands as a postulate or meaning rule, and T can no longer be claimed to be good, old-fashioned analytic.

We can go a long way toward defining color predications for non-uniformly colored objects by constructing a definition for "U(x)," a term which appears particularly susceptible to analysis. To say that x is uniformly colored is to say that x is *the same color all over*. Thus given the relation, "x is a part of y" ["P(x,y)"], which may be taken as primitive, or derived from a set-theoretical construction of surfaces, to say that x is uniformly colored is to say that all its parts are exactly the same color—that is,

$$\mathrm{U}(x) =_{\mathrm{def}} (y)(z)[\mathrm{P}(y,x) \cdot \mathrm{P}(z,x) \supset \mathrm{E}(y,z)] \qquad \mathrm{D}_3.$$

We may then say, in general, that if a color may be ascribed to a *part* of an object, it may also legitimately be ascribed to that object as a whole; namely,

$$(F)(x)(y)\ [\mathrm{Col}\ (F) \cdot F(x) \cdot \mathrm{P}(x,y) \supset F(y)] \qquad \mathrm{P}_2.$$

[9] We cannot accept Putnam's suggestion that "Col (F)" is to be translated, not "F is a color," but "F is co-extensive with a color," for then theorem T would leave open the possibility there might exist two different colors which are co-extensive and for which the principle of color incompatibility does not hold.

If, now, P_1 and P_2 were the only conditions under which color ascriptions could arise, we could define "*F* is a color" by

$$\text{Col}\ (F) =_{\text{def}} (\exists y)(x)\ [U(y) \cdot (F[x] \equiv (\exists z)\ [P(z,x) \cdot E(z,y)])] \qquad D_4,$$

from which, given the transitivity of "$P(x,y)$," both P_1 and P_2 may be deduced. D_4 states that any property is a color if and only if it is coextensive with a class, C, defined by the schema: an object is a member of C if and only if it contains a part which is exactly the same color as the uniformly colored object *a*. (Of course, Pap's criticism of the extensionality of D_2 applies here also, and the biconditional in D_4 is more properly understood as an intensional equivalence.) Thus D_4 exactly preserves the spirit of D_2, but extends color ascriptions to multicolored objects.

But we are not out of the woods yet, for aspects of our intuitive attribution of colors to objects still remain to be explicated. It will be noted that we have not restricted the range of "*x* is exactly the same color as *y*" to uniformly colored objects—otherwise D_3 would be circular. And of course it makes perfectly good intuitive sense to say, for example, that two checkerboards are exactly the same color or, at least, that they are colored exactly alike. We must therefore be prepared to cope with propositions about the color-samenesses of multicolored objects. In particular, "If *x* is colored *F* and *x* is exactly the same color as *y*, then *y* also is colored *F*," and "If *x* is uniformly colored and *x* is exactly the same color as *y*, then *y* is uniformly colored" would seem to be two truths just as apodictic as the classic case of color incompatibility. But counter-examples may be constructed which show that these propositions *cannot* be derived solely from D_1, D_3, and D_4, even in conjunction with axiomatization of "$P(x,y)$." A more extensive construction is called for.

Even in its present generality, moreover, D_4 does not extend color predication as far as is our intuitive wont. For under D_4, if an object is to possess a color, it must have at least one part which is exactly the same color as some uniformly colored object. But this is a strong stipulation. What about an object whose color varies continuously over its entire surface? It is difficult to say that such an object has any part whose color is exactly equal to that of some uniformly colored object, yet we would still not hesitate to say such an object is *colored*, albeit we might have trouble saying *what* its color is.

Finally, not only is D_4 too narrow for some purposes, it is also too broad in other respects. For just as its predecessor D'_2 did before it, D_4 admits predicates which would never be considered color terms.

So long as "*a*" is the name or description of *any* particular not multicolored, "E(*x*,*a*)" satisfies the definiens in D_4 whether *a* has a color or not, for if *a* has no color at all, it will satisfy "U(*x*)" trivially. Thus "*x* is exactly the same color as this B^b" qualifies under D_4 as a color predicate. Under D_4, *every* particular which does not vary continuously in color is colored; what we normally conceive as the lack of color becomes merely one of the color qualities. (This objection also applies to D_2, with or without contextual restrictions.)

III

Despite the vicissitudes which the line of analysis invoked by Putnam seems to encounter, it is far too soon to abandon hope for an analytic derivation of color incompatibility as expressed in theorem T. It seems to me—and this is my primary motive in exploring Putnam's construction so extensively—that we are now in position to achieve an entirely new (at least to me) insight into the use of color words. It appears that in natural language color qualities are ascribed to objects in two importantly different ways, one of which leaves theorem T empirically false, and the other which *does* make T analytic in the strict sense.

If one transcribes T into ordinary English, bearing in mind its contextual restrictions, one obtains, "If *F* and *G* are two different *specific shades* of color, nothing can be (at once) both *F* and *G all over*." The necessity for the qualification "all over" is obvious—it rules out the case where we predicate different colors of a single object whose parts are not all colored exactly alike. The need for "specific shades" is more subtle. The prima facie explanation is that it merely rules out *F* and *G* as the disjunctions of different classes of colors, in the sense in which we use "green" to mean any of a class of shades of green, for in this case the color of an object could be both *F* and *G* if these are the disjunctions of overlapping classes. Actually, to say that *F* is a specific shade of color is meant to have more force than this. It is intended to restrict T to the first of two ways in which *F* can be predicated of an object, one of which is *explicitly constructed* to let only one specific color be predicated of a uniformly colored object, whereas the other *does* permit a uniformly colored object to be of two different specific shades at once.

We have already seen that it seems perfectly acceptable, even though *F* and *G* are different specific shades of color, to say that an object is *both* *F* and *G*, so long as the object has differently colored parts. Very well,

then, let us construct a series of multicolored objects, each member of which is constructed from its predecessor by modifying the coloration of the latter in a specified way. For the first member of the series, we take, say, a square whose coloration divides it into four vertical stripes of equal width, alternately black and white. Let the rule of the series be that a member is generated by dividing each black stripe of its predecessor into two equally wide vertical stripes, leaving the right hand one of these black and recoloring the other white. Thus each member of the series is white except for two black stripes which diminish in width to vanishing as the series progresses. What, now, are we to say about the coloration of the series' *limit*? Each member of the series is both white *and* black, and yet the limit is uniformly white. The answer in this case, of course, is simple. For we do not merely say of each series member that it is black and white, we say that it is $p\%$ black and $100\text{-}p\%$ white. The limiting case may be said to be black and white only in the sense that it is 0% black and 100% white. And there is no difficulty in reconciling "x is 0% black" with "x is not black."

But what if we construct our series differently? Let the first member be the same as before, but this time let a member be formed by dividing *each* (black or white) stripe of its predecessor into two equally wide vertical stripes, coloring the right hand one black and the other white. Then each member of the series is characterized by alternating black and white stripes of equal width, with the width of the stripes decreasing to zero as the series progresses. And what are we to say of the coloration of the limiting case now? For the limiting case is obviously a uniform specific shade of *grey*, yet the predicate, "x is 50% white and 50% black" applies to every member of the series. It will not help much to reason that the *limit* of a series is not itself a *member* of the series and hence that while every member of the series is half white and half black, this does not mean that the limiting uniform grey must be called half black and half white. To argue thus is to introduce conceptual discontinuity where analysis finds none in fact. Further, there will be a finite cutting point in the series beyond which its members will be indistinguishable from the limiting grey—in fact, depending upon the further analysis of "$E(x,y)$," we could wonder whether the series might not even reach a member which is *exactly* the same color as grey. To reach closure on this point, we shall need some postulates about the divisibility of surfaces and the size limits to which color predicates are applicable, but it seems to me that we are inexorably driven to admit that if we can predicate "grey" of a uniformly colored object, we are

also justified in saying that it is also half black and half white (or perhaps some other mixture of black and white).

The extension of this line of analysis to the general case is obvious. It is probable that many, perhaps most, uniform specific shades of color can be produced as a limiting mixture of two or more other specific shades of color, where the proportion of a contributing color does not vanish in the limit. If A and B are different uniform specific shades of color which non-vanishingly intermix by some sequence the limit of which is the uniform specific shade of color C, it would seem that if an object x is colored C, there is *some* sense in which it is also proper to say that x is *also* colored A and B.[10] I wonder whether conceivably the reason we class a number of specific colors as, for example, "shades of *red*," might not be *because* the specific colors so classified can be compounded out of a prototypical shade of red; that we say a chartreuse object is also green and yellow because we can actually see chartreuse *as* a blend of yellow and green—that is, as *analyzable* into a yellow and a green without necessarily committing ourselves to which green and yellow.

But although I think our use of color terms for multicolored objects does allow ascription of more than one specific shade of color to a uniformly colored object, I would nonetheless agree that this is not the usual sense of "x is of specific shade of color F" when x is uniformly colored. When we say of x that it is red_6, we do not usually mean that red_6 shows forth in x among other shades, but that red_6 is the final composite of whatever shades may be abstracted from x. Thus while a uniformly chartreuse object is also in one sense prototypical green, it is in a different and more customary sense emphatically *not* prototypical green. It is in this second sense of being of a specific shade of color all over that theorem T is true, and moreover, good, old-fashioned analytically true.

To see how this comes about, observe first that full description of the color properties of a multicolored object requires not merely the listing of its colors, but their amount and distribution as well. The second

[10] This need not be as drastic a conclusion as it might seem. In view of saturation effects, etc., there are probably definite limits to the way in which one shade of color may be produced out of others. Note also that the mixing operations we have constructed here are *conceptual*, not physical. The fact that two pigments can be stirred together to give a third color is not germane for present purposes, since this is an empirical law about temporally successive events. But series such as the ones we have constructed reveal timeless, intrinsic relations and bring out the force of our color concepts.

black-and-white series we considered above reveals that patterning is a crucial ingredient in the composition of coloration, for it was by successive redistributions of the same amount of black and white that we generated a uniform shade, grey. In fact, examples such as this show that *just as the uniform shades of color fall on a multi-dimensional continuum, so do color configurations form an extension of that continuum,* an extension which is no less a legitimate part of the *color* continuum than the subspace containing the uniform shades. It seems to me, then, that "specific shade of color" has *patterning* built into it in addition to hue, saturation, and brightness, and that to say "x is of specific shade of color F" in the first sense is to say that F can be abstracted from the color properties of x, whereas to say that x is F in the second, more customary sense, is to say that F is the *totality* of x's color properties. Thus, in the first sense, a uniformly chartreuse object is also both green and yellow just as a checkerboard is both red and black. But also, just as a uniformly chartreuse object is neither (prototypical) green nor yellow in the second sense, neither is a checkerboard either red or black, for these terms apply (in the second sense) to uniform coloration.[11]

And how are we to analyze the property of being a specific shade of color in the second sense? The considerations just advanced expose a prerequisite for any acceptable explication. For if to say that x is F in the second sense is to describe the *totality* of x's color properties, then to say that x is also G, where "G" designates a totality of color properties different from F, is to utter a contradiction. It is a logical part of the concept of "totality" that describing F as a totality of color properties excludes the simultaneous possession of any other total set of color properties. (Compare "No class can have both a total of five members and a total of four members.") Therefore, if we translate "Col (F)" in theorem T as "F is a totality of color attributes," *it would seem to be a necessary condition for any acceptable explication of* "Col (F)" *that theorem T be revealed as analytic.*

As a matter of fact, if we are willing to take "F is a color attribute" in the first sense as a primitive second level property, it is not difficult to construct a definition for "F is a coloration-totality" which is both intuitively plausible and which logically entails T. Let "F is a color

[11] Note that there is nothing paradoxical about a uniform specific shade constituting a coloration totality for one object while forming only part of a more complex configuration or a component in another uniform specific shade for another object. Whether there are any specific shades of color which are "basic" in the sense of being unanalyzable into other shades is problematic.

attribute" be abbreviated "Ca(F)," and "F is a coloration totality" as "Col (F)." One definition of individual coloration totalities which immediately comes to mind is by reference to particulars through the relation, "x is exactly the same color as y"; for it is intuitively obvious that two objects are exactly alike in coloration if and only if they have the same coloration totality. Since to say that two objects have the same coloration totality is to say that any color attribute of one is also an attribute of the other, we may define "E(x,y)" by

$$\mathrm{E}(x,y) =_{\mathrm{def}} (\Phi)(\mathrm{Ca}[\Phi] \supset [\Phi(x) \equiv \Phi(y)]) \qquad \mathrm{D'}_1.$$

We might then define specific coloration totalities in terms of color identities with appropriate particulars—for example, "$\mathrm{Red}_6\ (x) =_{\mathrm{def}} \mathrm{E}(x,a)$"—and adopt D_2 as our definition of "Col (F)" with the added stipulation that y is to be a colored object. Theorem T then follows from D_2 and $\mathrm{D'}_1$.

But a number of objections can be raised to defining colors by reference to particulars, only one of which is that if $\mathrm{Red}_6\ (x) =_{\mathrm{def}} \mathrm{E}(x,a)$, "$\mathrm{Red}_6\ (a)$" is still (strict) analytic—which, as we saw in Section I, is objectionable not because of ontological commitments but because it does not allow us to say anything of factual content when we predicate red_6 of a. Fortunately, an alternative to D_2 is available which does not draw upon any particulars, and allows individual color words to occur as descriptive primitives. We note first of all that totality concepts are never themselves primitives, but logical constructions based on an empirical relation between properties and objects. When we say that Φ is the totality of x's properties of kind K, we mean not that Φ has an ontological peculiarity which makes it spurn cohabitation with others of its kind, but merely that Φ, which may elsewhere occur within a complex of K's, happens to be all the properties of kind K that x has—or, if Φ is itself a complex, that x happens to have no properties of kind K that do not always accompany Φ. Hence we may define "F is the coloration totality of x" ["$T(F,x)$"] by

$$T(F,x) =_{\mathrm{def}} \mathrm{Ca}\ (F) \cdot F(x) \cdot (\Phi)(y)[\mathrm{Ca}(\Phi) \cdot \Phi(x) \cdot F(y) \supset \Phi(y)] \qquad \mathrm{D}_5.$$

To say that F is a coloration totality is then merely to say that F is the property necessarily shared by all objects having a common totality of color attributes; namely,

$$\mathrm{Col}\ (F) =_{\mathrm{def}} (\exists\Phi)(x)[F(x) \equiv T(\Phi,x)] \qquad \mathrm{D}_6,$$

where the biconditional is best understood as intensional equivalence. To prove theorem T, we observe that by D_6, Col (F) · Col (G) ·

$(F \neq G) \cdot (\exists x)[F(x) \cdot G(x)]$ only when $(\exists \Phi)(\exists \Psi)(\exists x)[T(\Phi,x) \cdot T(\Psi,x) \cdot (\Phi \neq \Psi)]$. But the latter is self-contradictory, since by D_5, $(\exists x)[T\Phi,x) \cdot T(\Psi,x)]$ entails $(y)[\Phi(y) \equiv \Psi(y)]$, which in an extensional logic implies that $\Phi = \Psi$. (We obtain the same result for an intensional calculus by appropriate substitutions of intensional for material implication in D_5 and D_6.)

It should be noted that the present derivation of theorem T does not restrict its range to uniformly colored objects. The uniform shades of color are but a subclass of the coloration totalities, and a multicolored object can no more be of two different total configurations than a uniformly colored object can (in the second sense) be of two different shades.

IV

To summarize, the gist of my argument has been that while Putnam's explication of color concepts cannot sustain close inspection, partly for technical reasons and partly for its inability to deal with the general case, it would nonetheless seem that his analytic goal is still partly attainable. The present account appears to show that there are two different ways in which a specific shade of color F can be predicated of an object x; one in which we mean that F is *among* the color properties of x, and another in which we mean that F is the *totality* of these. It is only for the latter case that the law of color incompatibility holds. Moreover, while we customarily understand "x is F" in the second sense when x is uniform in color, we employ "x is F" in the *first* sense when x is divisible into differently colored parts and have no simple way at all of speaking about the coloration of an object whose color varies continuously from point to point. It would seem that our conceptual framework for color properties is not entirely in perfect health, and it is to Putnam's credit that his analytic scalpel has bared a hidden abscess.

But while the old enigma, "Nothing can be two colors all over at once" can indeed be shown to be good, old-fashioned analytic to the extent it is true at all, this is by no means the case for all the truths about colors which have so frequently been offered as prima facie examples of the synthetic a priori. In particular, we have here given no support whatsoever to the contention that "Red is a color" is analytic in the strict sense. For we have succeeded here *not* in defining "F is a color" ["Ca(F)"], but only in using "Ca(F)" as a second level *primitive* to define "F is a coloration totality." So far as the present

analysis is concerned, determination of whether any particular attribute is a color is a strictly synthetic judgment.

As a matter of fact, any argument which seeks to prove the analyticity of "Red is a color" by stipulation that colors are defined by color identity to appropriate particulars, with "E(x,y)" [or "I(x,y)"] taken as *primitive* simply begs the issue, for to say that two objects are the same in color is to *presuppose* a way of sorting properties into categories. To say of red circle *a* that it is indistinguishable in *color* from red square *b* is to identify the redness, rather than the roundness, of *a* as its color, and redness, rather than squareness, as the color of *b*.

Putnam has reiterated that he knows of no philosopher who has taken "Red is a color," or "Anything which is red is colored," as apodictic in any interesting sense. At the risk of exposing my philosophical naiveté, I must confess that I have long considered these to be the quintessence of synthetic a priori truth. I would hasten to agree, of course, that the synthetic a priori here is truth *ex vi terminorum*, and indeed, I would question whether there is a case of the former which is not also a case of the latter. I would also question, however, whether "true by virtue of the meanings of the terms" is properly equated to "true as a consequence of the rules of the language." But this is another story, and a long, involved one at that.

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A SEA FIGHT TOMORROW?

FEW men in the history of ideas have dared openly to flaunt the logical law of the excluded middle. This law decrees that for any declarative sentence,[1] either it or its negation, but not both, is true. In Chapter IX of *De interpretatione*, however, Aristotle, perhaps the first to give it clear formulation, attempts to show cause why this law must be restricted so that it does not apply to any sentence ϕ which affirms that a future contingency will take place. He grants (where "N" abbreviates "necessarily")

(i) $\ulcorner N(\phi \text{ v not } \phi)\urcorner$.

But he denies

(ii) ϕ is true

and he denies

(iii) $\ulcorner$not $\phi\urcorner$ is true

because he maintains that (ii) implies

(iv) $\ulcorner N(\phi)\urcorner$

and (iii) implies

(v) $\ulcorner N$ (not $\phi)\urcorner$.

And the admission of either (iv) or (v) entails the admission of (where "C" abbreviates "contingently")

(vi) $\ulcorner$not $C(\phi)\urcorner$,

which contradicts our original assumption,

(vii) $\ulcorner C(\phi)\urcorner$.

In essence Aristotle presents what he considers to be a paradox at the root of whose evil lies the law of excluded middle. On the basis of certain logical principles and an empirical premise he attempts to derive logically a contradiction the solution to which demands the overthrow of the law of excluded middle. Now this sort of affair is

[1] When we talk of sentences, we shall have reference to those of the declarative sort. Verbs employed in the present tense are to be interpreted tenselessly. And phrases such as "making a statement," "act of speaking," "this sentence occurs," and "to utter" are used indiscriminately to refer to the signalizing of sentences. "Corners" are used in the manner of W. V. Quine, *Mathematical Logic*, ch.i, sect. 6.

indeed what is often meant by "paradox," that is, a case where rules and premises customarily accepted as firm and certain are shown to lead to contradiction and thus to require modification. It will be my endeavor in this paper, however, to show that what Aristotle offers is not a genuine but only an apparent paradox, that is, that the logic employed in deriving the contradiction is faulty and thus no modification of basic principles is thereby necessitated. To this end I shall reconstruct Aristotle's arguments in an effort to render them sufficiently explicit and precise for objective evaluation. I trust that I shall depart only from their letter and not also from their gist.

We begin by accepting the following logical principles:

(1) a complete predicate calculus,

(2) if ψ is synonymous with or an explication of ϕ then ϕ implies ψ,

(3) ϕ is true or ⌜not ϕ⌝ is true and it is not the case that ϕ is true and ⌜not ϕ⌝ is true (i.e., the law of excluded middle).

In addition, we accept the following axioms (where "m" and "n" are variables ranging over times and "$<$" abbreviates "is temporally prior to"):

(4) ⌜$C(\phi) \equiv [\text{not } N(\phi) \cdot \text{not } N \text{ (not } \phi)]$⌝ (i.e., a familiar principle of modal logic),

(5) "A will be B (at n)" is true $\equiv$ A will be B (at n) (i.e., a formulation, sufficient for our purposes, of the correspondence theory of truth),

(6) $(m)\ (n)\ [(m < n \cdot$ it is the case at m that A will (not) be B at $n) \supset N$ (A will (not) be B at $n)]$.[2]

Let us pause at this point to comment upon (6). (6) may be idiomatically rendered as

(7) If, prior to an event, it is the case that that event will occur then that event is necessary.

[2] While it would obviate possible confusion to qualify the consequent of (6) to read "it is the case at m that N(A will (not) be B at n)," we shall here refrain from doing this for the sake of notational economy. The confusion might arise when, granting that a future event is at one time contingent and at a later time necessary, we erroneously think that we are committed to the *contradictory* view that one and the same event is both contingent and necessary.

In lieu of (6) we might have axiomatized

(8) $(m)\,(n)\,[(m < n \cdot$ it is the case at m that "A will (not) be B at n" is true) $\supset$ N(A will (not) be B at n)].

For (8) may be idiomatically rendered as

(9) If a sentence which affirms that an event will take place is true prior to that event then that event is necessary.

In his first argument Aristotle appears to appeal to (6) and in his second argument to (8), but I choose to treat (6) as axiomatic in both arguments for two reasons. First, (6) is more fundamental to Aristotle's thesis than is (8). The sort of necessity with which Aristotle is here concerned is that which accrues to an event by virtue of its occurrence; for once fact it is incapable of alteration.[3] In this sense of "necessity," any present or past event is ipso facto necessary, and any future event which is previously factual (whatever this may mean) must also be necessary. But this last is a formulation of our axiom (6). From this point of view, then, (6) is fundamental and (8) is a similar but elliptical way of speaking. Second (and perhaps this is little different from the reason just given), to treat (6) rather than (8) as axiomatic may render Aristotle's claim more convincing. For someone may say, "To me it just is not axiomatic that the prior truth of a future-tense sentence implies the necessity of what that sentence predicts." And perhaps if we state it object-linguistically, the principle will meet with a more intuitive reception. It may still be replied, however, "I do not understand what you mean when you say that it is the case prior to an event that that event will occur; what could 'previous factuality' or 'previous truth' possibly mean?" And at this point I am unable to come to Aristotle's aid.[4] But for the time being, we shall set aside the difficulties involved in the notions of previous truth and previous factuality; we shall grant Aristotle his axiom (6). And we shall endeavor to show that his arguments fail despite the gift, that his paradox never materializes because adherence to (3) involves attributing truth and not prior truth to future-tense sentences—and their truth does not logically entail their prior truth.

[3] Cf. G. E. M. Anscombe, "Aristotle and the Sea Battle," *Mind*, LXV (1956), 7-8, 12.

[4] Cf. Rogers Albritton, "Present Truth and Future Contingency," *Philosophical Review*, LXVI (1957), 29-46. Cf. A. J. Ayer, *The Problem of Knowledge* (London, 1956), p. 188.

An empirical premise employed in Aristotle's arguments is

(10) With regard to many affairs and events, there is future contingency.

That is to say that such affairs and events "may either be or not be"; "there is a potentiality in either direction."[5] And we hope to make it clear that contradictions in which instances of (6) and their negations (e.g., "C[*A* will be *B*]" and "not C[*A* will be *B*]") occur as conjuncts do not logically follow from this premise, certain innocuous suppositions, and the logical principles and axioms cited.

Let us now reformulate the first of three arguments offered by Aristotle.[6] Consider a statement, made at a time *m*, which affirms that a future contingency will occur at a later time *n*, say

(11) *A* will be *B*.

According to (3), either (11) or its negation, but not both, is true; and for simplicity's sake let us suppose

(12) "*A* will be *B*" is true.[7]

To state (12) more fully, in accordance with (2), we assert

(13) It is the case at *m* that "*A* will be *B* at *n*" is true.

From (5) and (13), by (1), we obtain

(14) It is the case at *m* that *A* will be *B* at *n*.

From (6), (14), and the stipulation that *m* is prior to *n* there follows, by (1),

(15) N(*A* will be *B* at *n*).

And from (4) and (15), by (1), we obtain

(16) not C(*A* will be *B* at *n*).

But we originally presumed

(17) C(*A* will be *B* at *n*).

Thus we have derived a contradiction consisting of an instance of (10), namely (17), and its negation (16).

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[5] *The Basic Works of Aristotle*, ed. by Richard McKeon (New York, 1941), *De interpretatione*, 19a 5-10.

[6] *Ibid.*, 18a 33—18b 9.

[7] It would make no significant difference in the argument if we were to suppose instead the truth of "*A* will not be *B*." "Further, it makes no difference whether people have or have not actually made [either of] the contradictory statements" (*ibid.*, 18b 36).

Now to elucidate the bad character of this argument. The usual connotation of a future-tense statement like (11) is twofold, pragmatic and semantic. The pragmatic connotation provided by "will" is

(a) The time at which this sentence occurs is prior to that for which it affirms the characterization of *A* by *B*.

The semantic connotation is

(b) *B* characterizes *A* at the time *n*.

Thus we render (12) explicit by saying that (a) is true and (b) is true. And this, as is certified by a correspondence theory of truth, is logically equivalent to asserting (a) and (b).

We may now observe that (13) is not an accurate elaboration of (12). For (13) is logically equivalent to (14), and the semantic connotation of (14) is not (b) but

(c) It is (already) the case at *m* (i.e., prior to *n*) that *B* characterizes at *A* the time *n*.

If (13) were an accurate elaboration of (12), then (12) would be logically equivalent to (14), which it obviously is not since (a) plus (b) does not imply (c). What Aristotle has done is to equivocate upon (12). When we assert that (11) is true, we mean to assert that what (11) affirms is the case, that is, we mean to assert (a) and (b). But to interpret (12) as (13) is subtly to incorporate the temporal reference of (a) into (b), thus transforming (b) into (c). It is one thing for a future-tense sentence to be true, and quite another for it to be *true at a certain time prior* to the event it predicts. And so Aristotle's first argument has produced a specious, not a genuine, paradox.

Aristotle presents a second argument[8] which may be reconstructed in the following manner. Suppose

(18) *A* is *B* at *n*

and that, prior to *n*, this event is a future contingency. Since the connotation of

(19) *A* will be *B* at *n*

is (a) plus (b), and since (b) is synonymous with (18), we may assert

(20) Prior to *n* it was always true to say "*A* will be *B* at *n*."

[8] *Ibid.*, 18b 10-17.

And to state (20) more fully, in accordance with (2), we assert

(21) (m) [$m < n \supset$ it is the case at m that "A will be B at n" is true].

From (5) and (21), by (1), there follows

(22) (m) [$m < n \supset$ it is the case at m that A will be B at n].

And, letting "m" name one of the times prior to n, we derive from (6) and (22), by (1),

(23) N(A will be B at n).

From (4) and (23), by (1), we obtain

(24) not C (A will be B at n).

But we originally presumed (17).[9] Thus, again, we have derived a contradiction consisting of an instance of (10) and its negation.

The *non sequitur* character of this argument may be brought out in the following way. Most simply stated it is that (21) is not an accurate explication of (20) as it purports to be. And a more accurate elaboration of (20) (which, as we shall soon see, must itself be revised) is

(25) (m) [$m < n \supset$ ("A will be B at n" occurs at $m \supset$ "A will be B at n" is true)].

(20) is quite acceptable. For in supposing (18) we have supposed (b). And if we include within the scope of our consideration the pragmatic connotation (a) of (19) as well as its semantic connotation (b), then it is important to add, as (20) does, the qualification that (19) is true if it occurs prior to n. But (21) does not make explicit (20)'s claim that if (19) occurs prior to n then (19) is true. Instead (enter equivocation) (21) makes the different claim that (19) is already true prior to n. This shift in subject matter almost passes unnoticed because (20)'s temporal reference to [(19)'s occurring at] *any time prior to* n is subtly incorporated into the semantic connotation of

(26) "A will be B at n" is true

as (26) occurs in the context of (21). That is, the "was" of (20) refers to acts of speaking and not to (26); but (21) reverses this reference, transforming the semantic connotation (b) of (19) and (26) into (c).

[9] For simplicity of argument, I avoid a more precise treatment in which (17) is temporally qualified in the fashion indicated at footnote 2, while the very same qualification is attached to (23) and (24) through the universal instantiation of (22).

If, contrary to this analysis, one were to insist that (21) is an accurate translation of (20) then we should have to point out that (21), and hence (20), does not logically follow from the preceding steps of the argument. When something is the case at a certain time, this entails the truth of any previous statement that the event in question will occur at this time; but it does not entail the *previous truth* of any previous statement that the event in question will occur at this time.

Hence either (21) is not an accurate elaboration of (20) and thus does not logically follow from (20), or (21) is an accurate elaboration of (20) and (20) does not logically follow from preceding steps in the argument. And if we accept (25) as an accurate elaboration of (20), the rest of the argument does not follow. Thus Aristotle's second attempt to construct a paradox fails.

We have seen that Aristotle's first and second arguments involve the fallacy of equivocation. And it is noteworthy that, even after this trick has been detected, we have only to reread Chapter IX in order to feel the force of Aristotle's arguments once again. I believe that this is due to linguistic habit. We are accustomed to such phrases as "he spoke softly," and "he spoke cheerfully." In the case of these and similar locutions, it is usual for the adverb to indicate a characteristic of the act of speaking; thus the temporal reference of the adverb is to the time of that act. And we employ identical grammatical form in "he spoke truly." Likewise it is usual for adjectives employed to characterize (token) sentences to have a temporal reference to the time of the token. And "that statement is gruff" and "it was mean to say" share grammatical forms with "that statement is true" and "it was true to say" respectively. In general, adverbs and adjectives usually possess the temporal reference of the verbs and nouns they modify.

I believe, then, that similarity of grammatical structure deceives us, that linguistic habit easily leads us to accept a similarity of temporal reference where we encounter similar grammatical form. Thus Aristotle's arguments mislead us into believing that since "he spoke truly" or "that (token) sentence is true" or "it was true to say," it follows that a certain sentence was or is true at a certain time. And in the case of future-tense sentences the tensed verb informs us that this time, the time at which the sentence is signalized, is prior to the predicted event. In this way we are induced to pass (easily but illegitimately) from the truth of a future-tense sentence to its prior truth. But despite misleading grammatical similarity, "true" and "truly" do not refer to a property of the act of speaking; they do not normally

have a temporal reference. Usually, they serve only to claim the factuality of that which is spoken of. When we say, for example, that he spoke truly yesterday, we mean not that what he said yesterday was true yesterday, but that he spoke yesterday and what he said is (tenselessly) true.

Perhaps the following objection will be raised to the tack pursued in this paper. "You admit that sentences like (11) have (a) as well as (b) for their connotation. This being so, the type

(27) Constantinople will fall in 1453

is true prior to 1453 and false at all other times. Hence in the case of future-tense sentences we must provide "true" a temporal reference. And if it is true prior to 1453 that Constantinople will fall in 1453, how could it be otherwise in 1453? Perhaps you will attempt to avoid this conclusion by tactics similar to those you employed in criticizing the second argument. You may say, that is, that if a token of the type (27) occurs prior to 1453 then the type (27) is (tenselessly) true. But this gambit ends in paradox and must be repudiated. For, *mutatis mutandis*, this principle permits us to say that if a token of the type (27) occurs at or after 1453 then the type (27) is (tenselessly) false. And upon the innocuous assumption that tokens of the type (27) occur both before and after 1453, it follows that the type (27) is (tenselessly) both true and false."

The fact that an objection such as this can be raised indicates that both (25) and (a) are in need of being made more precise. As they stand they leave room for doubt concerning when they refer to types and when to tokens. Indeed, as will soon be apparent, even after we make clear just what we intended by (25), (25) is not a correct way of stating this. No wonder, then, that this objection fells a straw man. But before going on to the task of making (a) and (25) more precise and so making our position clear, let me point out that the theory advanced by this objection will not do. It distorts what we customarily intend by predictions, for we usually mean by these to assert the future occurrence of an event and not to assert (as this theory would suggest) that the event is predetermined. Another of its disadvantages is that it would have a sentence to be true at one time and false at another. But the fundamental error of this theory is, as we shall see, a type-token confusion.

Now to suggest how future-tense sentences may be handled more precisely. Perhaps the following examples will indicate the sort of treatment which I have in mind. Suppose that one token of the

type (27) is uttered in 1400 and another in 1957. Let us call these "Token-1" and "Token-2" respectively. Each of these tokens must be translated differently. Token-1 will be explicated as

(a_1) The time at which Token-1 occurs is prior to 1453

plus

(b_1) Constantinople falls in 1453.

Token-2 will be explicated as

(a_2) The time at which Token-2 occurs is prior to 1453

plus (b_1). Now it is apparent that Token-1 is true and Token-2 is false.

Suppose that a token of the type

(28) This child will one day be handsome

is uttered in 1957. Let us call this token "Token-3." In this case the time of the predicted state of affairs is not specified. Token-3 will be explicated as

(d) There is a time at which this child is handsome and the time at which Token-3 occurs is prior to this time.

As these examples indicate, I am far from suggesting that we disregard the pragmatic connotation of predictions. There does seem to be something very like (a) involved in all future-tense sentences, and simply to ignore it would not do justice to common language. The pragmatic connotation temporally relates a certain spatiotemporal event, a certain token, to what is predicted by means of this token. It follows (with regard to future-tense sentences) that we cannot translate types but can only translate tokens, that translations of tokens must always refer to the token being translated and not (normally) to the type, and that only tokens and not types can be true or false. According to these criteria, we must repudiate both the theory suggested by the last objection and the interpretation of (25) which this objection attributed to us.

The tokens of each type of future-tense sentence are numerically distinct but qualitatively similar occurrences. They have, as we say, the same form. But since each such token is intended to mention itself, it must be provided with a unique translation. Thus these tokens are of the same grammatical type but not of the same type meaning-wise. In our present concern, therefore, we must avoid treating different tokens of the same grammatical type as if they were different tokens

of the same meaning-type. We shall not then have to say that the type (27) is true at one time and false at another, nor that the type (27) is (timelessly) both true and false. And we shall be less easily tempted by linguistic habit and grammatical form to pass unwittingly from the truth of a future-tense token to the necessity of what it predicts, although it may still require some effort to keep from attaching the time of a token to its predicates "true" and "false."

To avoid any misinterpretation of our critique of Aristotle's arguments, let us now recast this critique to accord with the foregoing discussion. Let us remove the type-token ambiguities of (a) and (25).

In the case of Aristotle's first argument, it is supposed that a token of the grammatical type (11) is uttered at m and predicts an event for n. We may call this token "Token-4" and translate it as

(a_3) The time at which Token-4 occurs is prior to n

plus (b). Then when we suppose the truth of Token-4, there is little temptation to suppose ourselves thereby committed to the position that Token-4 is already true prior to n. It should be clear that we are committed only to the view that Token-4 occurs prior to n and B characterizes A at n. And there are no grounds here to support the contention that the latter fact was already fact prior to n.

Aristotle's second argument supposes that A is B at n and then, translating the type (19), claims license to assert (20). We deny that future-tense types may be translated on the ground that reflexive reference to a particular token is part of the connotation of any future-tense sentence. But although such types cannot be translated, our knowledge of the general form taken by their tokens provides extra-systematic justification for our accepting, in the case of each type, axioms like the following (where "T" is a variable ranging over tokens) :

(29) $(m)\ (T)\ [(m < n \cdot T \text{ is of type (19)} \cdot T \text{ occurs at } m \cdot A \text{ is } B \text{ at } n) \supset T \text{ is true}]$.

And from (18) and (29), by (1), there follows (20), provided that by (20) is meant

(30) $(m)\ (T)\ [(m < n \cdot T \text{ is of type (19)} \cdot T \text{ occurs at } m) \supset T \text{ is true}]$.

But the analysis contained in the last paragraph applies to any token which fulfills the conditions of (30). It will now be seen that the shortcomings of (25) lie not only in its type-token ambiguities but also in its failure to generalize for all tokens of a certain type. And it is clear,

I think, that (30) does justice to the "to say" of (20) while (21) ignores this phrase.

Aristotle's third argument[10] proceeds as follows. "Since propositions correspond with facts, it is evident that when in future events there is a real alternative, and a potentiality in contrary directions, the corresponding affirmation and denial have the same character." Neither is yet true nor yet false. Hence "in the case of that which exists potentially, but not actually, the rule which applies to that which exists actually does not hold good." Otherwise stated: there are future contingencies; (3) demands that sentences predicting these be true or false prior to the event; there ensues the paradox that some events are both contingent and necessary; the solution of this paradox requires the restriction of (3).

We shall not have to prepare a brief against this argument, for it is less an argument than a statement of the thesis to the support of which the first two arguments have been directed. The whole question at issue has been whether or not (3) demands that future-tense sentences be true prior to the events they predict, whether or not the application of (3) to predictions of future-contingent events thereby leads to paradox.

By means of his first two arguments Aristotle has attempted to demonstrate that this is so, that we are enjoined, on pain of contradiction, to proscribe the law of excluded middle from a certain area. But we have seen that these arguments fail due to their commission of the fallacy of equivocation. And so we may conclude that Aristotle has given us no good reason for believing that we must restrict the law of excluded middle. For all that Aristotle's arguments demonstrate, one may adopt the law of excluded middle unreservedly and yet remain an indeterminist.

Having finished with the main task of this paper, I should like to essay an interpretation of Aristotle's intentions concerning those difficult notions to which we earlier referred as "previous truth" and "previous factuality." I suggest that Aristotle was misled by grammar into believing that truth, in the case of future-tense sentences, must be prior truth. Holding also that truth is correspondence with fact, Aristotle must have been impressed with the position that if we do not restrict (3) then, for any event, before it occurs it is a fact that it will occur.

But what can be meant by the apodosis of the last sentence? Does

[10] *Ibid.*, 19a 30—end.

it mean that causal conditions sufficient for the later occurrence of any event always prevail prior to that event? This is unsatisfactory: for we are led to this notion via a correspondence theory of truth, and when we say that a prediction is true we ordinarily mean to talk about a predicted event but not about its causes, although opinions about the latter may be causes of our so speaking. Does it mean that every event occurs before it occurs? But this is absurd. I do not think that this question can be answered in a way consistent with the truth of Aristotle's thesis. I do think, however, that he was led to advance this thesis not only by grammar and linguistic habit but also by the (only) apparent force of this unanalyzed apodosis.

There remain two questions to be answered. First, What does Aristotle mean by (i)? Given (i), then at least one of the alternatives must obtain. And in either case the shape of the future is settled. I believe that Aristotle would repudiate this conclusion. He would interpret each component as a statement of what happens in the future and not of what is going to happen in the future. And since it is the deceptive grammatical forms involving "true" and "truly" which lead him to shift from the former to the latter interpretation in the course of his arguments, he comes to reject (3). He comes to the conclusion that we must deny both (ii) and (iii).

At this point the second question arises. By what right do you say that, for Aristotle, both (ii) and (iii) must be denied? For he expressly says, "to say that neither the affirmation nor the denial is true, maintaining, let us say, that an event neither will take place nor will not take place, is to take up a position impossible to defend".[11] I interpret Aristotle upon this point in the following way. He denies (ii) and (iii) because he construes as prior truth the truth of future-tense sentences. But in the last-quoted passage, he interprets "neither the affirmation nor the denial is true" not as a denial of (ii) and a denial of (iii) but as the assertion that "an event neither will take place nor will not take place." And this, of course, he wishes to repudiate. He does wish to deny that it is previously true or previously fact that a future-contingent event neither happens nor does not happen in the future—for this, from his point of view, is yet another road to paradox, another way of maintaining that the future contains no genuine alternatives.

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[11] *Ibid.*, 18b 16-20.

ON "KNOWING HOW" AND "KNOWING THAT"*

THE distinction between "knowing how" and "knowing that," which Gilbert Ryle makes in Chapter II of *The Concept of Mind*, is the point of departure for this paper. Because of its simplicity and apparent obviousness the distinction has great appeal, but like any dichotomy it gives rise to much controversy and perplexity. Hartland-Swann has argued that "knowing that" can be reduced to "knowing how."[1] I believe his reduction holds if "knowing how" and "knowing that" are used to refer to a rather limited range of dispositions. I also believe, however, that once "knowing that" is reduced to "knowing how," a distinction must be made between two types of dispositions subsumed under "knowing how." In the first section of this paper I hope to point out the types of dispositions to which Ryle, and hence Hartland-Swann, refer when they use the phrases "knowing how" and "knowing that." In the second I shall discuss the nature of the new distinction which must be made once we grant Hartland-Swann's reduction. Finally, in the third section I shall examine certain "knowing how" and "knowing that" sentences which neither Ryle nor Hartland-Swann considers, in order to discover whether or not the dispositions to which they refer can be subsumed under Hartland-Swann's "knowing how."

1. In order to formulate Ryle's distinction between "knowing how" and "knowing that" as clearly as possible, it is necessary to ascertain the meaning of the terms "knowing how" and "knowing that." Ryle calls "know" a capacity verb and thus I think it is safe to conclude that he would call both "knowing how" and "knowing that" capacities also.[2] (Ryle differentiates capacities from tendencies although both are dispositions. A tendency implies not only that something *could* be the

* I would like to thank Dr. Israel Scheffler of Harvard University for his valuable criticisms of earlier versions of this paper and for suggestions which were extremely helpful in making revisions. The responsibility for error is, of course, my own.

[1] John Hartland-Swann, "The Logical Status of 'Knowing That'," *Analysis*, XVI (1956), 111-115; and " 'Knowing That'—A Reply To Mr. Ammerman," *Analysis*, XVII (1957), 69-71.

[2] To be precise, he states that "knowing how" is a disposition but never makes explicit the "logical status" of "knowing that."

case, but that it *would* be the case regularly when the appropriate conditions are realized; a capacity implies the ability to do something under specified conditions but does not imply frequency or regularity.) At no time does he say exactly what he means by the two types of knowing. From the examples he adduces and several of his statements, however, I think it is possible to determine that "knowing how" refers to skills or operations, for example "knowing how" to play chess, "knowing how" to theorize, "knowing how" to speak Russian; and that "knowing that" refers to one's "cognitive repertoire," that is, to knowledge of factual propositions, as for instance, "knowing that" Sussex is a county in England, "knowing that" *Messer* is the German word for knife.

It is essential to note that Ryle assimilates all "knowing how" to the model "knowing how to perform a task" and all "knowing that" to the model "knowing that such and such is the case," for we then realize that his distinction is of a more limited nature than we might at first have thought. For the fact is that in ordinary language the phrase "knowing how" is often used when performances are not involved, and the phrase "knowing that" is found in sentences which do not refer to knowing factual propositions. For example, we say "Johnny knows how a motor works," "I know how Eisenhower felt on election night," and "Jones knows how the accident happened." We also say, "Smith knows that he ought to be honest," "The child knows that he should be quiet when someone is speaking," and "Johnny knows that stealing is bad." None of these examples fits Ryle's paradigms for "knowing how" or "knowing that."

To summarize, Ryle's distinction between "knowing how" and "knowing that" is really a distinction between "knowing how to perform skills" and "knowing propositions of a factual nature." When Hartland-Swann discusses the question of the reducibility of "knowing that" to "knowing how," he too, I believe, is viewing "knowing how" and "knowing that" in this way. Thus in discussing his reduction one must not assume that it holds for all "knowing that" sentences. In fact, I think we will find that such sentences as "Johnny knows that he ought to be quiet" and "Jones knows that he should be honest" cannot be reduced to Ryle's and Hartland-Swann's "knowing how." This problem will, however, be discussed in Section 3. First I will discuss those sentences to which Hartland-Swann's reduction applies.

2. Hartland-Swann maintains that Ryle's distinction between

"knowing how" and "knowing that"[3] proves to be unstable when subjected to analysis. Every case of "knowing that," he says, is a case of "knowing how." This follows from the fact that "know" is a dispositional term. If I understand him correctly, what he means is that if we call the statement "Johnny knows that Columbus discovered America" dispositional, then it must be translatable into some such form as "Johnny knows how to answer the question 'Who discovered America?' or 'What did Columbus discover?' correctly." The only alternative to this inclusion of "knowing that" in the "knowing how" category, Hartland-Swann feels, would be to give up the dispositional analysis of "know."

I think one must agree with Hartland-Swann that a dispositional analysis of "knowing that" entails a translation of a "knowing that" sentence into a "knowing how" sentence of the type illustrated above, that is, knowing how to answer a question or to state a fact. I think, however, it would be a mistake to end the analysis of "knowing" with this reduction, for granted that "knowing that" can be reduced to "knowing how," there is still a fundamental distinction to be made within Hartland-Swann's new, expanded "knowing how" category. The basis for this distinction, I think, lies in the fact that two very different sorts of dispositions are subsumed under "knowing how."

Let us consider for a moment the case of Jones who was witness to the murder of Y. Without doubt Jones knows that X murdered Y, and this, in turn, means he knows how to state that X murdered Y and knows how to answer the question "Who murdered Y?" Yet it seems intuitively obvious that there is an essential difference between his knowing how to answer the question "Who murdered Y?" and his knowing how to swim or speak French. That is to say, the difference between the capacity involved in "knowing how to state that X murdered Y" and the capacity involved in "knowing how to swim" is more basic than the difference between the capacities involved in "knowing how to swim" and "knowing how to do logic," or in "knowing how to ice skate" and "knowing how to play the violin."

I would like to suggest that the feature which distinguishes these two kinds of capacities from each other is *practice*. That is, "knowing how to swim" is a capacity which implies having learned how to swim through practice; "knowing how to answer the question 'Who murdered Y?' " is a capacity which does not imply having learned how to

[3] In this section "knowing that" and "knowing how" will be used in the narrow sense discussed above; i.e., "knowing that" will refer to knowing factual propositions, and "knowing how" will refer to knowing how to do something.

answer the question through practice. When Jones was a witness to the murder, he knew immediately that X murdered Y and did not need to practice stating facts or answering questions. Similarly, when Jones looks out his window and sees rain falling, he knows that it is raining without any sort of practice in saying "It is raining" or answering the question "What is the weather like right now?" To be sure, if he knows that it is raining, he *is able* to state certain facts and answer certain questions, but his capacity to do so does not imply that he has practiced doing so. On the other hand, Jones could not know how to swim or speak French unless he had at some time practiced swimming or tried to speak French. If Jones tells us he knows how to swim we are justified in asking him if he has ever tried to swim. If he answers "No" to our query, his assertion will be discredited. But if Jones tells us that he knows that X murdered Y, it surely would be nonsensical for us to ask him if he has practiced that assertion or tried to answer questions on the subject before.[4]

If, as I propose, the difference between the two types of capacities subsumed under "knowing how" is based on the notion of practice, some interesting consequences follow. If knowing how to swim requires learning to swim through practice, then it is unlikely that the practice itself is "swimming." The practice may consist in kicking and arm-waving and, if all goes well, these will gradually approach swimming. Although the point at which the practice in swimming becomes swimming is not for us to determine, it is interesting to consider the case of the individual who practices just up to the point where he actually swims and then gets out of the water. I think we could say of him that he knows how to swim even though he has not yet actualized this capacity by swimming.[5]

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[4] With regard to the notion of practice, a possible misinterpretation must be forestalled. I do not mean to say that knowledge of factual propositions is always learned instantaneously. Very often exposure to a variety of experiences and a protracted period of drill is needed before an individual knows that such and such is the case. However, the effort involved in learning and hence knowing that such and such is the case cannot be considered practice in *stating* that particular fact or *answering* a particular question. Moreover, to make the point that knowing factual propositions does not imply having learned to answer questions about them through practice, one case in which practice does not occur is sufficient; the example of Jones knowing that X murdered Y is surely such a case.

[5] It should be noted that there can also be cases of knowing factual propositions (i.e., knowing how to state certain facts or answer certain questions) which are never actualized. For example, Jones may know that X murdered Y

Just as there may be cases of knowing how to swim which are not cases of swimming, so there may be cases of swimming which are not cases of knowing how to swim. For example, it is conceivable that Jones falls into the water one day and swims to shore rather efficiently although he has never practiced or tried to swim before. We cannot deny that he is swimming but we might well wish to deny that he knows how to swim. In the case of swimming, of course, it is logically possible but in fact unlikely that there would be a performance of the skill which had not been preceded by practice. If, however, we think of a skill such as hitting a target, we realize that it is not too unusual for a novice to hit the bull's-eye without any previous practice. In such a situation we would maintain that although he hit his mark he does not "know how" to hit it. For we would expect someone who knows how to hit the target to hit it again. In other words, hitting a target is an occurrence which may be due to accident or luck; knowing how to hit a target is a capacity, and we would be right to look for a certain degree of consistency of behavior.[6]

It appears, then, that although Hartland-Swann's reduction of "knowing that" to "knowing how" is legitimate for those "knowing that" sentences which are cases of knowing factual propositions, there is still a basic distinction between these sentences and the kinds of "knowing how" sentences which are cases of knowing how to perform an operation. Whether or not it is agreed that the basis for the distinction is practice, I do not think the distinction itself can be denied.

3. In this section I would like to suggest the beginnings of a classification of "know" dispositions. I have proposed that Hartland-Swann's expanded "knowing how" category be divided into two distinct types of dispositions. I would now like to examine the two types of sentences mentioned in Section 1, examples of which are "Jones knows how the accident happened" and "Johnny knows that he ought to be honest,"

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but never reveal his knowledge to a soul. Thus he has the capacity to state that X murdered Y although he never actualizes it.

[6] It should also be noted that there may be cases of *stating* factual propositions which are not cases of *knowing* them. Thus, Johnny may state that X murdered Y because he has made a lucky guess and not because he knows that X murdered Y. I am aware, of course, that "practice" is a vague term. Although I do not think its limits need be set here, it is important to realize that many skills are related and that practice for one skill may thus serve as practice for another. Hence, on those occasions when it appears that we know how to do something without having practiced it, upon reflection we will discover that we have had practice in a related skill.

in order to discover whether or not these represent dispositions which can also be subsumed under Hartland-Swann's "knowing how" category. It is important, I think, to analyze the various types of "knowing how" and "knowing that" sentences in ordinary speech and to make such differentiations as are necessary, even if the simplicity of a reduction or a dichotomy is thereby lost. For just as Ryle has pointed out the dangers inherent in the reduction of "knowing how" to "knowing that," one may also point out dangers inherent in a reduction of "knowing that" to "knowing how" if analysis is discontinued at that point. It would seem no more desirable to teach mathematical or historical facts as if they were skills like swimming than to teach swimming as if it were Latin or geometry. And an equally grave mistake would be to teach moral judgments and rules of conduct as if they were either Latin or swimming.

Once we perform Hartland-Swann's reduction, we have a "knowing how" category which includes two distinct types of capacities. One type implies that the capacity has been learned through practice whereas the other does not. Let us call the capacities which require practice Type A, and the capacities which do not require practice Type B. Capacities of Type A, then, are skills or competences such as cooking, doing logic, playing tennis, and hitting targets. Although knowing how to perform each skill is, of course, quite different, I think these capacities can be classified together by virtue of the fact that they all require practice. Capacities of Type B are the capacities to state certain facts or answer certain questions correctly, that is, knowing in the sense of knowing factual propositions. These, I have argued, do not require practice.

Let us now examine sentences of the type "Jones knows how the accident happened" and "Johnny knows how Williams hits the ball so hard." Do they fit into the expanded "knowing how" category? From a linguistic standpoint these sentences surely belong under "knowing how." A linguistic criterion is not sufficient, however, for "knowing how the accident happened" must be a capacity if it is to be subsumed under Hartland-Swann's "knowing how."[7]

[7] It might be argued that "Johnny knows how the accident happened" can be translated into "Johnny knows that the road was slippery, that the car skidded," and so on, and that in this translated form the sentence is an instance of Type B. That is, it is an instance of knowing a factual proposition which, in turn, is reducible to knowing how to state a fact or answer a question. Whether or not such a translation is legitimate, however, I think we must consider the untranslated sentence on its own merits.

Since "knowing how the accident happened" is a case of "knowing," there can be no doubt that it is a disposition. Moreover, since it implies the ability to do something and does not imply frequency or regularity, it must be a capacity and not a tendency. But what type of capacity is it? "Knowing how the accident happened" is not a case of "knowing how to perform an operation." Moreover, it is a capacity which need not be learned through practice. Certainly if Johnny sees the car skid on the ice and crash into a tree he will not need to practice anything at all in order to know how the accident happened. It would seem, then, that "knowing how the accident happened" is a disposition which does not belong to Type A. Perhaps it belongs to Type B. To be sure, "knowing how the accident happened" appears to involve knowing factual propositions. Indeed, this is why practice is not required. However, the statement "Johnny knows how the accident happened" is less informative than a statement of the Type B variety. Its very broadness renders it uninformative. We know only that Johnny has certain knowledge which presumably could be elicited from him upon demand. But we do not know the specific details of his knowledge, which in turn means that we are not given the specific details of the way the accident happened. A Type B sentence, whether it be "Johnny knows that X murdered Y," "Johnny knows that Columbus discovered America," or "Johnny knows that the road was slippery and the car skidded," gives us a greater amount of information because it is more specific and thus excludes a greater number of alternatives. Moreover, it gives us information about two things at once: about what Johnny knows and about the factual proposition which he knows.

We must conclude, then, that dispositions such as "knowing how the accident happened," "knowing how Eisenhower felt on election night," and "knowing how the motor works" belong in the "knowing how" category but are distinct from Types A and B. That is, there is a third type of capacity, which we will call Type C, which is subsumed under "knowing how."

Let us turn now to the last type of sentence to be discussed here, sentences which refer to knowing moral judgments or rules of conduct. Linguistically, sentences which refer to knowing rules of conduct are "knowing that" sentences, for we say "Johnny knows that he ought to be honest" and "Johnny knows that he should be quiet when someone else is talking." There seems to be a difference, however, between these "knowing that" sentences and the "knowing that" sentences which refer to knowing factual propositions. For although a response at a verbal level informs us that Johnny knows that Columbus discovered

America, it does not inform us that Johnny knows that he should be quiet: the evidence of Johnny's knowledge that he should be quiet must be presented at the level of action or conduct.[8]

Since we do not believe that Johnny knows he should be quiet unless he behaves in certain ways, it is evident that "knowing that he should be quiet when someone else is talking" cannot be reduced to "knowing how to state a fact" or "knowing how to answer a question." Perhaps it is reducible to "knowing how to perform an operation" and belongs with capacities such as "knowing how to swim" and "knowing how to do logic." I do not think so, because I believe that "know," in the case of moral judgments and rules of conduct, acts as a tendency word rather than a capacity word. Thus the sentence "Johnny knows that he should be quiet when someone else is talking" is different from "Johnny knows how to swim" because Johnny must be quiet quite regularly when someone else is talking before we will attribute the disposition to him.

I realize that analyzing sentences of the type "Johnny knows that he ought to be honest" or "Johnny knows that he should be quiet in class" as tendencies may give rise to sharp disagreement. It will be argued that we often say "Johnny knows that he should be quiet while someone is speaking but often he is not," and that this is incompatible with the position that the sentence expresses a tendency. We would not, for instance, say, "The dog has a tendency to howl when the moon shines but he almost never does." We would consider this a contradiction. To be sure, we would, in attributing a tendency, permit a small number of lapses. That is, we could say, "Johnny knows he should be quiet while someone is talking but he interrupted the teacher today," just as we could say "Fido has a tendency to howl when the moon shines but last night he didn't." In either case we would seek out the factor which prevented the actualization of the tendency. We might find that Johnny interrupted the teacher because he smelled smoke, and that Fido did not howl because he had become engrossed in a juicy steak bone. Neither lapse would cause us to disbelieve in the

[8] Whether "knowing that" functions in the same way in the sentence "Johnny knows that Columbus discovered America" and in the sentence "Johnny knows that he ought to be honest" depends, perhaps, on one's view of "ought" and other value words. However, I think it is important to differentiate between the use of "knowing that" in factual propositions and in moral judgments or rules of conduct, or else we would be committed to the view that Johnny knows he ought to be honest even if he never does an honest act in his life.

tendency. But this does not solve our problem. The fact remains that we do say "Johnny knows he should be quiet, but usually isn't." How, then, can we call sentences of this sort statements of tendency?

I think this problem can be solved and that the root of it lies in the fact that our language is ambiguous. When we say, "He knows that he should," we sometimes mean one thing and sometimes another. We may mean by "He knows that he should" that he knows he is supposed to, or is expected to, or will be whipped if he doesn't. In this sense I think that "should" is used to express a factual statement. Thus, "Johnny knows that he should be quiet" may mean "Johnny knows that his teacher expects him to be quiet" or "Johnny knows that the rules say he is supposed to be quiet," and so forth. These are statements that rightfully belong under "Knowing how, Type B." If we view them as instances of Type B, we find it not at all surprising that Johnny knows that he should be quiet but isn't. For knowing, in the sense of Type B, is a capacity which may be exhibited in behavior at the verbal level only, that is, by answering a question or stating a fact correctly. It would be a mistake indeed to expect Johnny to be quiet if his knowledge of this rule of conduct were of the Type B variety.

Although by "He knows that he should" we sometimes mean "He knows that such and such is the case," we can also mean something quite different. We may use "Johnny knows that he should" to indicate that Johnny has internalized a certain rule of conduct or moral code. When we use "He knows that he should" in this sense, we expect that the behavior will, quite generally, be exhibited. This sense of "He knows that he should" is a statement of tendency and it would be a contradiction to say, "He knows that he should but he usually isn't." Thus, whether or not "He knows that he should be quiet but usually isn't" is contradictory depends on what is meant by "He knows that he should." On the one hand, if the phrase is to be taken in the sense of Type B, then we have no contradiction. That is why the phrase sounds so natural to us. But if the phrase is to be taken as an attribution of a tendency, then there is a contradiction.

To summarize the discussion of knowing rules of conduct, it would appear that since this type of disposition is a tendency it cannot be subsumed under the "knowing how" category. For that category as set up by Ryle, expanded by Hartland-Swann, and subdivided here contains capacities and not tendencies. Thus another category of "knowing" is needed, a category which contains tendencies. What the name of this new category should be I will leave an open question. For although "Johnny knows that he should be quiet" is a "knowing

that" statement of tendency on the surface, it might well turn out that it can be reduced to a "knowing how" statement of tendency.

The analysis of "know" dispositions presented above is in no sense intended to be exhaustive. My purpose has been merely to discuss four types of "knowing how" and "knowing that" sentences and to classify them in a way that seems both precise and useful. This endeavor has led to the conclusion that a category containing capacities of the "knowing how" variety will not account for all "knowing that" sentences and that another category, containing tendencies, is needed as well. Whether or not all "knowing how" and "knowing that" sentences can be subsumed under one or the other of these categories and whether or not other distinctions can be made within the two categories are questions which require further analysis.

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ON SOME PROPERTIES AND RELATIONS OF IMAGES*

It is my purpose in this paper to defend the assertion that images are existent entities of some kind and that they have certain properties that enable us to relate them to physical objects. I will defend the way of talking about images as having properties against a person like Professor Gilbert Ryle, who would like to say that there are no images to have properties, or perhaps that there are images but that they are not the sort of things that have properties.[1] This argument, in a nutshell, is that in imaging something I do not see (have, observe, introspect, and so forth) a special kind of private entity—an image—that represents the object in the way a picture represents an object, but it is only correct to say that I imagine I see the object. But surely these two accounts are not the mutually exclusive statements that Professor Ryle makes them out to be. He seems to consider that if we can establish that it is "correct" to say that we imagine that we see objects this will then guarantee that we do not do this by means of a private representative entity—the image. Yet the statement "I am imagining the facade of my house by having or entertaining images of the facade of my house" makes perfectly good sense, as does the following conversation. Fellow of the Royal Society: "I am imagining a castle." Mr. Francis Galton: "How are you doing this?" Fellow of the Royal Society: "Why, by having (entertaining, conjuring up, and the like) an image of a castle which I then observe." Consider also the extensive experiments of Heinrich Klüver on imagination, where he showed that if people are told to imagine a horse, or any other object or scene, and then to hold the image as fixed as possible (translation: to go on imagining the same horse for as long as possible), they find that after a few seconds the image undergoes a variety of transformations such as movements, transpositions of parts, fragmentations, distortions, and so forth, quite out of the voluntary control of the observer. Surely it would be very strange to deny the usage that "the image has broken into parts" in favor of the usage that we can only imagine for any

* Based on a paper read to the Philosophical Society, Oxford, on February 22, 1957. This work was done while the author held a Nuffield Fellowship in Medicine.

[1] *The Concept of Mind* (London, 1949), ch. i.

length of time horses that break (or that seem to break) into parts. The mere fact that the description "I am imagining X" fits what I so do at times in no way excludes the possibility that the description "I am having (seeing)[2] an image of X" fits the same events; nor particularly that the description "I am examining an image closely" may not be used by a psychologist who happens to be interested, for example, in a particular sequence of changes that eidetic images may undergo under different circumstances. It is the common-sense view that images *are* mental pictures and certainly it seems strained and unnatural to suppose, when we introspect a clear little hypnagogic image that has suddenly appeared out of the darkness just as we were dropping off to sleep, that this clear and glittering little image is not an existent entity of some kind which has suddenly swum into our ken.

Professor Ryle (in a letter) here objects that I should make up my mind which verb I am going to rely on, "for 'entertain,' 'have,' 'see,' 'inspect,' 'introspect,' 'observe,' etc. are all very different." These verbs, however, as well as others such as "examine" and "gaze" are all actually used by people interested in reporting their sensory images. Introspective psychologists use terms like "inspect," 'introspect," or "examine"; ordinary people will use "see," "look," "gaze." "Entertain" is possibly pedantic and Victorian; "have" is merely a more general term which may subsume any of the others. Different verbs have been evolved to deal with different aspects of veridical vision; why should not more than one of these verbs be used to describe different aspects of the rich phenomenology of hallucinations and of human relations to them?

There is one more relevant passage in *The Concept of Mind*: "When a person says that he 'sees' something which he is not seeing, he knows that what he is doing is something which is totally different in kind from seeing."[3] I should like to dispute this on two grounds. First, from my experience with normal people having hallucinations induced by hallucinogenic drugs, I should say that these people were particularly impressed by how similar "seeing" is to seeing. I am classifying the so-called mescaline hallucinations (or sensory images) here with hypnagogic-like images, for there is a clear-cut connection between hypnagogic images and mescaline images, and an even clearer connection between after-images and mescaline images, for the former may turn directly into the latter. In any case I think that Professor Ryle

[2] If the image is a sensory image (see below), use either "having" or "seeing"; if only an ordinary "mental" image, use only "having."

[3] *Ibid.*, p. 246.

would have to say that we "see" hallucinated objects and that we do not see them. Second, if "seeing" is really so totally different from seeing, it is surely surprising that people use exactly the same word for each occasion. In the case of many hallucinations of the class called "apparitions" (in which we must use "seeing" according to Ryle and in which equally "seeing" must be totally different from seeing), the hallucinated "person" is frequently mistaken by the percipient for the living person, and this mistake may only be discovered months later when evidence is obtained which shows that the person was abroad or dead at the time. In these cases, clearly, the person did not know he was really only "seeing" when he would have said at the time that he knew very well he was *seeing*; how so if "seeing" and seeing are *totally* different? Even in many cases when the person knows he is only "seeing"—because Uncle Harry is in South Africa or in the grave—the event simply *is* very like seeing Uncle Harry ("I saw him as clearly as I am seeing you now"). Furthermore " 'seeing' Uncle Harry" may be differentiated by percipients from *not* "seeing" Uncle Harry by descriptions such as "sensing his presence," and so forth.

I submit then that the common-sense account of imaging used also by introspective psychologists is a perfectly valid one. But what are the difficulties about it which have led Ryle to reject it? Let us lead up to this by considering now some general properties of images. There is one overridingly important property of (visual) images obvious to introspective analysis and that is their spatiality: images are *spatial* entities. That is to say that one part of an image bears spatial relations, such as "to the left of," "above," "below," "near," "nearer to," "farther from," to another part of the same image. One image may bear these same spatial relations to another image. Furthermore, an image has a *boundary* that limits its extension, and images have geometrical as well as topological properties. The angles of an imaged triangle (consider a hypnagogic image) add up to something like 180 degrees (depending on whether image space is Euclidean or non-Euclidean); one pair of imaged lines may be much more nearly parallel than another pair; the boundary of an image usually forms a Jordan curve, and so on.

Here an objection might be made that images do not have spatial properties but that they are only images of objects and it is the objects that have the spatial properties. This again, however, seems a strained account of the matter when we are confronted with an image. Furthermore, images may possess different spatial properties or relations to those possessed by the objects. For instance, a visual image, even of a

solid object, is always a mere surface; that is, it is only one surface bounded by a single closed curve, whereas the object that the image may represent consists of a volume bounded by a surface. When we are having flat images, as we do, of solid structures, we are not imagining flat maps, plans, paintings, and the like, of these structures. In any case we are not constrained to make up our minds at this time. Ryle may be right in saying that there are no images in the sense that there are objects. But it is also possible that what at first sight seems obvious—that there are images which have spatial properties and relations—is correct, and it is certainly worth while to explore the implications of the fact that images may have spatial properties and bear spatial relations. And we can say at once that this fact is clearly incompatible with Descartes' idea that the mind is necessarily and essentially unextended, lacking all spatial attributes—spatiality or extension being the cardinal attribute of *res extensa*—for mental images are surely of the mind mental, and spatial images cannot belong to an unspatial mind. How then are we to relate images to objects? As images and objects are both spatial entities, in the sense of having spatial properties and bearing spatial relations, could we then relate images to objects spatially?

We can distinguish three possibilities: (1) images bear spatial relations to other images but they do not bear spatial relations, but only causal relations, to physical objects—the theory put forward recently by Professor H. H. Price;[4] (2) images bear spatial relations of some kind and causal relations both to other images and to objects; and (3) we can say that it is meaningless to suggest that images do or do not bear spatial relations to objects. This last argument may be advanced in two ways. (a) The first is that we cannot use words like "round," "between," and so forth, in regard to images, for these words were taught us at that inescapable position—our mother's knee—by pointing at physical things; thus their meanings are fixed with regard to physical things and not images; and thus it is illegitimate to use these words in contexts logically different from those in which we learned them.

We can, however, reply to this objection as follows: (i) such words are constantly used successfully with reference to images, particularly by introspective psychologists, and (ii) the use of these words could perfectly well have been taught with reference to images (such as eidetic images in children) and might even have been done in that way had

[4] *Aryan Path* (1956), 41, 87.

we belonged to a culture that valued afterimages or images beyond objects. These meanings could then have been transferred to objects. (b) The second is a much more sophisticated objection and was raised by Professor John Wisdom at a recent meeting of the Moral Science Club in Cambridge. It runs as follows: "It is true that images are spatial and that physical objects are spatial, yet they are spatial in different senses, for the meanings of the statements 'images are spatial' and 'objects are spatial' lie in the means of verification of these statements. As these means of verification are different in the two cases, the meanings of 'spatial' are different in each case in such a way that, whereas it makes sense to say 'Images bear spatial relations to other images' and 'Objects bear spatial relations to other objects,' yet it does not make sense to assert that images either do or do not bear spatial relations to objects." Now let us examine this argument more closely. How do we verify the statement "this object is spatial"? We can either look again or we can touch it, feel it, and walk round it to confirm its spatiality. But we cannot touch, or feel, or walk round an image; we can only look again. Thus the means of verification are different.

The reply to this objection is threefold: (1) It may be claimed that the meanings of the statements "images are spatial" and "objects are spatial" do not lie in their means of verification, for it would seem to be a necessary condition for any verification procedure to be such that there should at least be the logical possibility of failing to verify the statement, or that we could understand what it would be to fail to verify the statement. Consider, for example, what it would be to fail to verify the statements "this iron is heavy" or "light travels at 186,000 miles per second." Suppose then we saw an object and said "that object is spatial" and then put our hands out to touch it and found that the "object" was in fact hallucinatory. Would we then say that the percept that we had taken to be the object—the hallucinatory percept—was not itself spatial? Surely merely touching the object does not alter the properties of the first percept we had of the "object" (or even object) in any way. Imagine further a man examining a clear-cut and long-lasting hypnagogic image. He might say, "That is a hypnagogic image beyond any shadow of a doubt and it is spatial," and on being pressed to examine it again by the psychologist at his bedside he might say, "No, I can see I was mistaken, it has not changed at all but I now observe that it is not spatial." To this statement we are tempted to retort, "Nonsense, if it *was* a hypnagogic image unmistakably when you first observed it, of course it still is spatial." But even if we accept it, does the truth or falsity of this statement have any

bearing on the truth or falsity of his first statement? Surely the meaning of every statement descriptive of present experience lies in the present experience described. The statement "That afterimage is round" means that that afterimage is now round there, and nothing that is done, or can be done afterwards, can alter the truth or falsity of this statement or can juggle its meaning. (2) Even if we accept Wisdom's argument, this surely only enables us to conclude that we cannot walk round, or touch, or feel images, and it does not necessarily enable us to conclude that images are spatial in such a sense that they cannot be said to bear spatial relations to objects. For we can say the following: (a) even if the different verification procedures do show that images are spatial in *some* way different to that in which objects are spatial, this does not necessarily show that they are different in the particular way of being logically constrained from bearing any spatial relations to each other. (b) The state of affairs described in the accounts that Wisdom is here objecting to surely describes a *possible* universe; for example, there is no contradiction involved in saying that images either do or do not bear spatial relations to objects, and any attempt to prove the impossibility of such a universe on purely logical grounds would seem to be irrelevant. (3) The relevant point is surely that, if images do bear spatial relations to physical objects, how would we then verify this relation? I have suggested elsewhere[5] how this could be done.

Thus in conclusion we can suggest that images are spatial entities that may bear either spatial or causal relations, or both, to physical objects.

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[5] J. R. Smythies, *Analysis of Perception* (London, 1956).

LANGUAGE AND PHILOSOPHY

In his article, "Verbs and Times" (*Philosophical Review*, LXVI, 1957, 143-160) Mr. Zeno Vendler argues that the nature of philosophical categories may be deduced from the nature of language, in particular that "verbs have tenses."

Unfortunately he seems to identify "language" with the English language. He appears unaware of the fact known to all comparative linguists, namely, that, with unimportant exceptions, *only* Aryan (Indo-European) languages possess tenses, that is, verb inflections that indicate past, present, or future time. The Chinese language, for example, is entirely uninflected and finds no need for any tenses. It gets along quite well by specifying time, when desired, by an adverb, such as "now," "yesterday," and so forth. Might it not be advisable for linguistic philosophers to study more than one type of language before attempting to derive philosophy from linguistics?

Homer H. Dubs

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Reply to Professor Dubs

The aim of my article is far less ambitious than Professor Dubs obviously supposes it to be. I merely proposed that the classification of *English verbs* can be refined by taking into consideration the time-schemata implied by their use. I also pointed out that with this improvement the policy of dealing with certain philosophical problems along linguistic lines may be rendered more effective. Since such a policy has been widely pursued by more capable hands, the burden of its justification does not rest with me alone. Yet I venture to offer a specific reply to his main point.

Philosophical problems, in order to be treated at all, have to be formulated in *some* language or other. In case it is English, the analysis of this language may be helpful; if it is Chinese, that tongue is the relevant one. Now it might happen that solutions given in English fail to solve anything in Chinese and vice versa. But then, I ask, are the problems the same? How can one decide the issue? "Translate them,"

one might suggest. Yes, but what if the literal translation does not bring out the problem? "Then translate it so that the problem appears." Then, I say, "translate" the answer in such a way that it takes care of the problem.

No doubt, among languages of similar structure, say within the Aryan group, problems and answers carry over from one language into another without much trouble. And yet some modifications might be required. For instance, in German I could not argue from the difference between verbs that do and those that do not admit continuous tenses. In Hungarian, which is not an Aryan language and which is, incidentally, my native tongue, most of my arguments would break down in this sense. By the way, in what sense are the tenseless Chinese "verbs" verbs? Is their concept of action the same as ours?

In this connection, I think, it would be extremely interesting to investigate the "family resemblance" of philosophical problems and the analogy of their treatment as they are found in various languages. For this "second-order" task the study of comparative linguistics would be indeed indispensable.

ZENO VENDLER

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BOOK REVIEWS

THE FOUNDATIONS OF SCIENCE AND THE CONCEPTS OF PSYCHOLOGY AND PSYCHOANALYSIS. Minnesota Studies in the Philosophy of Science, Volume I. Edited by HERBERT FEIGL and MICHAEL SCRIVEN. Minneapolis, University of Minnesota Press, 1956. Pp. xiv, 346. $5.00.

Two major preoccupations give this book its distinctive character: the bearing of general principles of methodology upon psychology, and the three-cornered argument between logical empiricists, behaviorists, and psychoanalysts. There can be no doubt that psychology is an odd science, and readers concerned with diagnosis and possible therapy could well begin with the short paper by one of the editors, modestly placed at the end. Michael Scriven's "A Possible Distinction between Traditional Scientific Disciplines and the Study of Human Behaviour" could have served an an introduction. It states part of the problem and gives part of the answer. But the plan is otherwise. The two first papers, by Feigl and Carnap respectively, are concerned mainly with more general issues in the philosophy of science. The remainder are devoted to more special questions in psychology and the philosophy of mind.

Feigl reviews some developments in logical empiricism in the last twenty-five years. It is a story of liberalization. The early positivism had some of the features of an ideology with a party line. But doctrines previously dismissed as due to culpable stupidity are now more seriously considered as possible insights vouchsafed to genuine seers, albeit seers of somewhat clouded vision. Liberalization is achieved partly by discrimination and partly by moderation. Discrimination is displayed in the new criteria proposed for factual meaningfulness. Occam's razor, once freely used as a weapon in philosophical gang warfare, is now replaced by Occam's pocket knife, a useful tool with three distinct blades. Moderation is displayed in the concession that the criterion of factual meaningfulness is to be construed as a proposal, not as a proposition, and in the conclusion, "Even if we cannot (and should not) claim to have arrived at a philosophy to end all philosophies it may perhaps be acknowledged that the new enlightenment has made considerable advances."

The process of liberalization is further illustrated in Carnap's

important paper on "The Methodological Character of Theoretical Concepts." Modern psychology continues to make increasing use of "dispositional" concepts. This paper contains an illuminating discussion of the relation of "Theoretical Terms" to "Pure Disposition Terms." In the course of this discussion, Carnap refers to the possibly unduly restrictive influence of empiricists and operationists upon psychologists. In consequence, some psychologists became overcautious, "others whose methodological superego was fortunately not strong enough to restrain them dared to transgress the accepted limits but felt uneasy about it." Perhaps, however, Carnap opines, those who hold empiricists responsible for this overestimate the influence of philosophers on scientists in general. But surely the point is that it is the psychologists in particular rather than the scientists in general who have suffered from their influence. The greater freedom now allowed by empirical theory is accordingly all the more important for psychology. The danger perhaps is that psychologists will find it hard to adapt themselves to this greater freedom. Some will find Carnap's statement that "the total rejection of introspection was unwarranted" a very hard saying. They may be shocked to read that "a person's awareness of his own states of imagining, feeling, etc., though limited by its subjective character, must be recognised as a kind of observation in principle not different from external observation." It is as though a devout congregation were told from the pulpit that sin is not so serious a matter as had often been supposed.

In large measure, however, the psychologist's chains have been fashioned by himself. His masochistic self-discipline has arisen from his own faulty diagnosis of the malady of his science. Only rarely have difficulties arisen from concern with "subjective data." Psychologists do not often disagree about descriptions of feelings and mental images. They *know* and agree that in such experiences individuals differ. They are at least as likely to disagree about the description of behavior and about the concepts employed in theories about behavior. Witness the major disputes in the volume—the difference between Skinner and Scriven, between Buck and Miller, indeed between Skinner and Freud. None of these disputes turns on the subjectivity of the data. Disputes about introspection do not arise from faulty introspection but from faulty, unclear thinking; and thinking can be unclear whatever the thinking is about.

Skinner's "Critique of Psycho-analytic Concepts and Theories" opens the three-cornered fight. It is a polemic against Freud's account of the "mental apparatus." The task was not difficult but is well

performed. It is a good thing to have the indictment so clearly presented. But the case is largely presented in terms of Skinnerian behaviorism and Skinnerian theory about theories in general, and so is exposed to a Scrivenian critique of Skinnerism. Scriven's "Study of Radical Behaviourism" is detailed and devastating. It sufficiently establishes the point that "Those who cry 'no politics' often thereby support bad politics, and those in whose prefaces philosophy is abjured often proceed to expound bad philosophy"; also that those who say "no theories" present indefensible theories. It spotlights the point that Skinner attempts to defend a thesis which is exciting, controversial, and practically indefensible by arguments which would only support another thesis which is moderately interesting, widely accepted, and very plausible.

The three-cornered argument is continued in Albert Ellis' "An Operational Reformulation of some of the Basic Principles of Psycho-Analysis"—a worthwhile exercise but one which falls between two stools. In some ways it is a bit too radical, in others not radical enough. The danger in such translations is that of throwing out the baby with the bath. Some of those here offered retain the baby and some of the dirty water, too. At least some cloudy water remains in the form of translations not much less obscure than the original text.

Antony Flew's "Motives and the Unconscious" is in the nature of a postscript to a long argument conducted in the pages of *Analysis*. It concentrates on the thesis that if you extend the meaning of such words as "motive" to cover "unconscious motives" you can interpret more human behavior in terms of concepts of this sort than had been previously realized. Flew adds, however, that the paper is not concerned with the truth of analytic theories. The conjunction of these statements is a little puzzling. The extension of concepts *in principle* presents little difficulty. That is common scientific practice. Justification of the extension surely depends on the truth of the statements made in terms of these extended concepts.

Chronbach and Meehl's "Construct Validity in Psychological Tests," and Meehl's "Problems in the Actuarial Characterization of a Person," are important reprints deserving an extension of the comparative immortality insured by inclusion in this volume. They carry the application of general methodological principles to more specialized fields of psychological research. Meehl and Sellars' paper on the "Concept of Emergence" calls for reference to an earlier paper by S. C. Pepper, but ties in with Sellars' longer contribution which forms the penultimate piece in this volume. No brief commentary could do

justice to this long, entertaining, original though a little obscure discourse, which ranges widely not only over most of the fundamental topics of the volume, but also, in greater detail, over the various solutions proposed for "the sense data problem." Its thesis is sufficiently indicated by the title of the original lecture as delivered in London: "The Myth of the Given." It is to be hoped that later volumes of this series will provide an opportunity for the adherents of the myth when they have rallied their defenses to mount a counterattack.

R.C. Buck's delightful critique of supertheoretical theories (not too happily sandwiched between Meehl and Sellars, and Sellars) entitled "On the Logic of General Behaviour Systems Theories" is a sort of complement to Scrivens' critique of Skinner's anti-theoretical theories and the same author's terminal essay. The logical positivists may have much to answer for but they are not responsible for the sort of "physicalism" ("the physicalizing of psychology") criticized in this paper. Psychologists would seem to fall broadly into three classes, according to their distinctive types of orientation: to theory, to method, or to fact. The first two types have been unduly influenced by the model of physics. The third group has been in the main concerned with ascertaining causal factors which influence selected variables, for example delinquency, the rate of forgetting, aesthetic preferences, and so forth. They mostly employ simple methods and are not much concerned with the larger theories. They are not much concerned with precise predictions or with establishing "laws." When their findings can be expressed in laws, these laws are of the form of an alternation of conjunctions: p is a function of either $(a + b + c \ldots)$ or of $(f + g + h \ldots)$ or . . . where neither the conjunctions nor the alternatives are complete. These empirical psychologists are very content if they have accounted for a substantial part of the variance of p. The hypnotic influence of physics is pervasive and appears in unexpected places. Even Scriven appears to be somewhat overconcerned with his possible distinction between physics and psychology. In not including the basic biological sciences among the traditional disciplines, he tends to present as a sharp dichotomy what is in fact a continuous variable—the extent to which practical problems are soluble. Working psychologists, like honest-to-god biologists, are concerned with prediction to a very limited extent and then only in terms of probabilities. The major point in this admirable paper is indeed of much less interest and importance than a number of other points thrown out in a casual way. There are many important implications in the observation that psychology is almost unique among the

sciences in having had much of the scientific cream skimmed off by "common sense" before the science begins. Psychologists, too, will owe a lot to Scriven if they allow him to wean them from the messianic hope of having a Galileo or a Newton of their very own.

In this as in several other contributions a breath of fresh air is let into a number of stuffy holes and corners, and maybe the prospects of progress both in psychology and in the philosophy of mind are rather brighter than they were before this volume appeared.

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TIME AND MODALITY. Being the John Locke Lectures for 1955-56 delivered in the University of Oxford. By A. N. PRIOR. Oxford, Oxford University Press, 1957. Pp. x, 148. $4.00.

Professor Prior's John Locke Lectures are, he tells us in the preface, the expression of a conviction that formal logic has more to bring to general philosophy than is sometimes supposed. What then is the philosophical significance of the topics he takes up and of the observations he has to offer on them?

No explicit answer is given in Prior's work. Several possible reasons, however, are suggested by his discussion.

(1) In the first chapter, Prior seems to conceive of his task as a study of the common features of a variety of formal systems which he calls "modal." From this point of view, he is able to demonstrate the rationale of the apparently strange systems of "basic modal logic" and "Ł-modal logic" developed by the late Jan Łukasiewicz. The idea is not pushed much further, however, and its philosophical significance is not obvious.

(2) In the next few chapters, Prior examines analogies between certain systems of tense-logic and the well-known Lewis systems $S4$ and $S5$ of ordinary modal logic. It seems to the reviewer that the interest of these analogies is overestimated. For one thing, the perfect analogy which the book makes one think there is between $S4$ and one of the tense-logics does not hold. (This, I have been informed by Professor Prior, has been pointed out by Mr. E.J. Lemmon also.) The reason is fairly obvious: since the moments of time are linearly ordered, there could only be an analogy between tense-logic and modal logic if the different "possibilities" studied in modal logic were also linearly

ordered. This would mean that of any two "parallel" possibilities one should comprise the other: *Mp* and *Mq* should imply *MKpMq* or *MKqMp*. There is no reason, however, for thinking that this is the case. It follows that the tense-logical "analogue" to S4 can be formalized by adjoining to the axioms of S4 the further axiom *CMpCMq CLCpLNqMKqMp*.

At this point, it may seem as if the whole enterprise of building a logic of tenses is rather trivial. All there is to time is a one-dimensional continuum, and the logical properties of such structures have been studied extensively.

(3) It is a great merit of Prior's to have pointed out an important purpose which the study of tense-logic may serve. If by "there is" we mean "there now is," attempts to combine tense-logic and quantification become interesting. They are interesting because they give rise to problems similar to those one encounters when trying to combine quantification with ordinary modality. Mastering the difficulties of the latter kind is, it seems to me, vital for our understanding of modal notions in general. The difficulties may be easier to overcome in tense-logic than in modal logic proper, for the individuals existing at different moments of time present much smaller philosophical difficulties than the "possible individuals," "intensions" and other entities to which logicians have sometimes resorted in order to make sense of quantified modal logic.

The problem of combining modality-like temporal operators with quantifiers is posed in a particularly sharp form by the fact that the most straightforward way of combining the two makes provable formulae which are intuitively false. Prior eliminates these by modifying radically the underlying tense-logic (which is analogous to S5). It may be questioned, however, whether the resulting system Q should be compared with S5 in the way Prior compares them or whether it should rather be considered as an extension of a system which is still very much like S5. In fact, there is a sub-system of this kind imbedded in Q: the combination of symbols *NMN* behaves with respect to *M* and to the propositional connectives almost exactly like the *L* of S5. In fact, the only thing in Gödel's well-known formulation of S5 that does not remain valid if the *NMN* of Q is substituted for *L* and the *M* of Q for *M* is the axiom *CLCpqCLpLq:* here it is no longer allowed to substitute a formula for *q* unless all its variables are already in the formula substituted for *p*.

For the purpose of getting rid of the objectionable formulae, there-

fore, most of the peculiarities of Prior's Q are unnecessary. Moreover, the smaller system seems somewhat closer to ordinary usage than Q. When one says that Jones has always been quarrelling with his brother and always will be, one does not imply that Jones and his brother enjoy sempiternal existence. One merely says that as long as there have been and will be Jones and his brother, they have had, and will have, arguments with each other. Examples of this kind show that, in ordinary language, "always" behaves more like Prior's *NMN* than like his *L*.

Setting up Q seems motivated only if we want to express certain things which are not expressible in the smaller system. Among these is the notion of sempiternal existence as well as the notion of the existence of an individual. Whether these should be expressible in a logical system is perhaps not as obvious as it may first seem. The second, for example, smacks of the much-criticized idea that "existence is a predicate." Be this as it may, the way in which Prior strives to explicate the notion of the existence of an individual seems to me mistaken: the relatively common-sense idea of the existence of an individual is explicated by means of the highly artificial notion of the existence of facts.

For a similar reason, I find it difficult to pass a judgment on the interesting suggestions Prior offers for alternative ways of constructing a quantified logic of tenses. They are based on Lesniewski's "common-noun" logic which presupposes quantification over something not unlike predicates. Just how much philosophical weight procedures of this kind can carry is at present a moot question.

(4) In addition to the formal analogies between temporal and modal notions, there may be real connections between the two groups of notions. For example, past events may be said to be necessary in *some* sense of "necessary." There arises, therefore, the problem of formulating the relation of this notion of necessity to temporal notions in such a way that present and future events are not all necessary. This problem is relevant to the philosophical problems connected with determinism. The difficulties into which Prior's valuable discussion of the problem leads strongly suggest that the many-valued logics he uses may not always be the best possible tools in modal logic.

(5) The logic of tenses may also be of considerable historical interest. In an extensive appendix, Prior discusses the views of some nineteenth-century logicians on the subject. His quotations from Peirce are especially interesting. And it seems to the reviewer that there are still bigger fish to be caught here. Prior refers casually to

what he calls the third truth-value of Aristotle. A recent paper by Miss G.E.M. Anscombe in *Mind* makes one doubt, however, whether this designation is at all correct. In any case, there is much more in Aristotle bearing on the subject of Prior's book. Among other things, the extant premises as well as the conclusion of the celebrated Master Argument of Diodorus (which is reconstrued and analyzed by Prior) are all found in Aristotle.

From what has been said, it will appear that Prior succeeds in making good his original promise: he takes up several topics of genuine philosophical interest. In a pioneering venture like his, it is only natural that many things remain unsaid and that many alternative ways—including, it seems to me, the most important ones—remain unexplored.

Professor Prior has reported to me also that, in the large diagram on page 124, *ML* should be connected with *MLM*, and that *P* on page 132 should be *p*.

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SCIENTIFIC INFERENCE. By Sir Harold Jeffreys. Second edition. New York, Cambridge University Press, 1957. Pp. viii, 236. $4.75.

The first edition of this work appeared in 1931, but it has been largely rewritten for this second edition, which is in effect a new book. A new discussion of statistical mechanics and quantum theory has been added, as well as discussion of a number of philosophical views about scientific procedure. But Sir Harold's general point of view remains unchanged: he disapproves of any attempt to interpret scientific method as an arbitrary game or as a manipulating of useful fictions; he insists that scientific hypotheses should be described as probably true. The guiding thread of the book is "that scientific method can be understood if and only if a theory of epistemological probability is provided" (p. viii), and that by means of the theory of probability we can "tidy up the process of scientific inference," which is "reasonable on the whole" (p. 22).

Just as he did in his *Theory of Probability*, Sir Harold here follows Keynes (whom however he nowhere mentions) in insisting that probability must be construed as a logical relation between data and a hypothesis, a relation representing the degree to which the data support the hypothesis. This logical relation cannot be defined, he insists

(p. 23); and Carnap's elaborate explication of it he dismisses without discussion as "prematurely formalized" (p. 185). Sir Harold is still at war with the frequency theory of probability, which attempts to define probabilities as empirical facts about infinite series of events, and he shows that it is "a network of begged questions" (p. 183), its definition of probability being one according to which we never could legitimately make any judgments about probabilities at all.

How is Sir Harold's form of probability to be employed in the confirming of scientific laws? He makes use of Bayes's theorem, which asserts that new evidence increases the probability of a hypothesis in proportion to the prior probability of the hypothesis and according to the likelihood of the new evidence. This theorem indicates that if we are to use observational evidence in order to confirm a hypothesis we must be entitled to assume the hypothesis to have some finite initial probability, prior to all observation. Many writers have balked at the thought of assigning finite probabilities to all hypotheses in the absence of any empirical evidence; but Sir Harold has no patience with such squeamishness. He claims that finite initial probabilities can be assigned to all empirical hypotheses in a satisfactory way; and to this end he asserts (p. 36) a postulate to the effect that "the set of all possible forms of scientific laws is finite or enumerable, and their initial probabilities form the terms of a convergent series of sum 1." (In asserting that the sum of the initial probabilities must equal one, he is perhaps thinking of a set of mutually exclusive hypotheses rather than of the set of all hypotheses.) All hypotheses of the "same form" must be "counted together," and the ordering of hypotheses is to be effected in terms of simplicity, the simpler hypothesis always being assigned the greater initial probability. But what is it to call one hypothesis simpler, less complex, than another? Sir Harold says, "We could define the complexity of a law as the sum of the absolute values of the integers in it, together with the number of adjustable parameters" (p. 39). This scheme is not very promising, however, for it does not enable us to distinguish among the degrees of complexity of non-numerical hypotheses; it has, moreover, the unseemly consequence that the relative complexities of hypotheses would depend upon what system of units happened to have been chosen. But Sir Harold does not insist on this particular scheme: if it be thought too arbitrary, he proposes that the matter be referred to an international body of scientists who would decide how the complexity of hypotheses is to be rated—a remarkable proposal for, contrary to what the book elsewhere seems to say, this suggests that the choice between rival hypotheses

is a matter of convention, like the choice between, say, Fahrenheit and Centigrade.

As it stands, Sir Harold's doctrine about initial probabilities seems rather less than convincing, for it is only one among many possible schemes. Besides the initial probabilities, moreover, the problem of confirmation as he has posed it to himself involves another aspect: the accumulating of favorable observational evidence needs to be able to increase the probability of a hypothesis above its initial probability and toward one as a limit. One would have expected that Sir Harold would attempt to show, in terms of his scheme, why the probabilities of certain kinds of hypotheses do increase in this way as certain kinds of evidence are accumulated. But he says nothing whatever about this. Furthermore, he makes no attempt to show how his abstract scheme of probabilities could be brought to bear upon concrete cases in which we choose between rival hypotheses. It would have helped if we could have been given clear examples of people making mistakes due to their poor grasp of probability, mistakes which they would not make had they this better grasp of its logic. Because of the lack of examples, this earlier part of the book has a somewhat arid abstractness.

In the next portion of the book, Sir Harold seeks to follow out what he takes to be the implications of his theory. In the light of his principle of inverse probability, he holds that the more precise the inferences given by a law, the more its probability is increased by their verification; but all observational evidence must be regarded as involving a margin of error. So in chapters on error and on sampling some theorems about probability are developed which are relevant to this. Chapters on physical magnitudes and on mensuration then discuss the manner in which quantitative judgments about observed phenomena are arrived at: he discusses for instance the way a scale of length may be constructed using a screw, so that the length of an object may be correlated with a certain number of turns of the screw. But he blandly bids us choose a screw every turn of which "is known always to have the same length" (p. 82)—a proviso which unfortunately means that his account cannot give us any very fundamental insight into the nature of judgments about length.

The latter part of the book deals with actual topics in physics: Newtonian dynamics, light and relativity, statistical mechanics, and quantum theory. Here Sir Harold is at his best, and readers with some knowledge of mathematics who desire a better grasp of fundamental ideas of physics should find these chapters rewarding. The continuity

between classical and modern physics is stressed; Sir Harold explains for instance that the relativistic conception of simultaneity should not be regarded as so radical an innovation, for the naive notion of simultaneity had had to undergo a more profound alteration long before in order that the observations of Jupiter's moons might be accounted for. These chapters on physics are largely independent of the earlier discussion of the probability of scientific hypotheses, and this fact in no way diminishes their value.

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FAITH AND KNOWLEDGE. A Modern Introduction to the Problem of Religious Knowledge. By JOHN HICK. Ithaca, Cornell University Press, 1957. Pp. xix, 221. $3.50.

This book is an essay in the epistemology of religion. It attempts to tell us what religious faith is and to show that religious faith is, or can be, a form of knowledge. Its standpoint is said to be "philosophically to the 'left' and theologically to the 'right': that is to say, it looks for enlightenment in the directions of philosophical analysis and theological neo-orthodoxy" (p. v). It may be theologically to the "right," but it is misleading to say that it is philosophically to the "left." For although it is partially clothed in the idioms of recent analytic philosophy (it "unpacks" concepts), its basic epistemology is about as far to the "left" as that of Josiah Royce.

The book consists of four parts. Part I is concerned with the "nature" of knowledge (ch. 1) and belief (ch. 2). Part II is a critical treatment of three theories of faith, the voluntarist theory (ch. 3), the moral theory (ch. 4), and the theory of the illative sense (ch. 5). Part III (chs. 6-8) sets out the author's own theory of faith. And part IV (ch. 9) is an interpretation of Christian faith in the light of the foregoing. Chapters 1, 6, and 7 contain the heart of the argument.

Knowledge, we are told, is subjective rational certainly. Rejecting the "infallibilist" claim that to know is to be directly and infallibly acquainted with reality or truth, on the ground that where knowing can be said to be infallible it always turns out to be uninformative, Hick opts for a "non-infallibilist" theory, but one which is not the usual alternative to the "infallibilism" which he finds untenable. "Knowledge" Hick calls "a diploma word." We award it to "that kind or aspect of cognition which is for us final and normative. This aspect . . . is the experience of absolute psychological certainty"

(p. 13). He cites with approval Locke's statement that "to know and to be certain is the same thing." He then distinguishes between "objective" certainty and "subjective" certainty (almost always a dangerous move) and concludes that the certainty in question must be subjective certainty, for "all our actual moments of certainty must, just because they are *ours*, be instances of subjective, personal, or psychological certainty" (p. 17). Thus knowledge is subjective certainty.

There are many confusions here, but two points must suffice. Of course "to know and to be certain is the same thing," but this is not the important psychological insight which Hick takes it to be. It is not a psychological truth at all, but a linguistic truism. What it tells us is that wherever we can say "I know that *p*" we can say instead "I am certain that *p*," or that it would be logically odd to say "I know that *p*, but I am not certain that *p*." It does not follow from this that when I say "I know that *p*" I am saying "I am subjectively certain that *p*." What does the "subjectively" contribute? Presumably "I am subjectively certain that *p*" means the same as "I *feel* certain that *p*." But "I am certain that *p*" and "I feel certain that *p*" are clearly not the same; they have quite different uses.

Secondly, take the argument that all our certainties *must* be subjective simply because they are *ours*. This sounds like the old argument for ethical egoism: all of our actions are selfish simply because they are ours. This is again surely to misconstrue a truism, namely, that I cannot say "I am certain" without using the first person singular pronoun.

Faith, according to Hick, is a genuine form of knowledge. Of course it must be, for it is a form of subjective certainty. But there is more to it than this, for there is more to knowing than mere subjective certainty. To know something, Hick claims, is to apprehend it as significant in some way, that is, to interpret it. To know that something is a book is to interpret it, to see it as significant; to feel moral obligation is to interpret a practical situation; and to have religious faith is to attribute a certain significance to *everything*. Faith is a "total interpretation."

Again the difficulties are legion. Not the least of them is to grasp what Hick means by the correlative terms "significance" and "interpretation." To interpret is to attribute significance to something. But what is significance? "By significance I mean that fundamental and all-pervasive characteristic of our conscious experience which *de facto* constitutes it for us the experience of a 'world' and not of a

mere empty void or churning chaos" (p. 112). I cannot see what this could possibly mean, nor do the examples which Hick gives serve to enlighten me; on the contrary they strike me as just so many equivocal uses of the vague terms "significance" and "interpretation."

And what are we to do with faith as a "total interpretation"? The only sense I can make out of this is that religious faith is just acting (including talking) *as if* there were some powerful but loving demon behind the scene. (Cf. p. 129 and the parable on pp. 150-151.) But this will not satisfy Hick. He does not like the idea that there might be equally reasonable alternative "total interpretations." In particular, he does not like Naturalism, acting as if there were no one behind the scene or as if the world were not scenery at all. And yet he realizes that a *total* interpretation, by definition, is peculiar. "It differs from all intramundane interpretations in that there is nothing outside and beyond the interpretand itself which might serve to throw light upon it" (p. 134). And so, although he claims that "theism is not an experimental issue" (p. 145), he is driven into making use of the bizarre device of eschatological verification to save the day: we shall see when we die who is right, the naturalists or the theists. But what could experience after death possibly contribute that experience now cannot? The dead cannot be *epistemically* any better off than we are.

This book is by no means all dross, but its basic approach to the problem of faith, which has been tried before, will probably convince only those who are already convinced.

W.E. KENNICK

Amherst College

THE COMING WORLD CIVILIZATION. By WILLIAM ERNEST HOCKING. New York, Harper and Brothers, 1956. Pp. xiv, 210. $3.75.

Large in perspectives but not in bulk, this eloquent book is "a philosopher's approach to history." Its theme is the development and future of modern civilization. Professor Hocking begins with an interpretation of "the experiment of modernity" over the past three or four hundred years: the attempt to rear a civilization upon secular foundations. Despite some remarkable achievements, the experiment is proving tragically inadequate. The secular state, whether it is dealing with crime, education, the family, the economy, the arts, or recreation, depends for its health upon a motivation and moral integrity which it cannot itself evoke. There must be a morale of con-

sent for a free state to function, and the police state, with all of its might, cannot provide a viable substitute. Nor can a purely secular culture.

Tracing "modernity" back to its spiritual roots, Hocking finds a split-mentality emerging from Cartesian philosophy. "Once Descartes had defined his two loci of certitude—for he had two, the I-exist and the mathematical rigor of nature's processes—the way was invitingly open to set up a system, either of pure objectivism (as of Hobbes) or of pure subjectivism (as of Malebranche and Berkeley)." Instead of rejecting the bifurcation or trying to follow either alternative exclusively, modernity has tried to grasp both horns. The result is to produce a schizoid civilization.

Objectivism has led to the conception of nature as "value-purged"—purposeless, qualityless, deterministic. Subjectivism has led to egocentricity. Neither alone, nor both in combination, can supply the inner spiritual cohesion without which civilization will perish. With the rapid penetration of occidental ideas and ways into the East, the exigency of Western civilization is becoming universal. Hocking nevertheless proposes to cling to "*both* advances of modernity," the gain in subjective depth and in objective certitude, while recapturing a *third* dimension of being, religious intersubjectivity. He thus hopes to heal the "split-mentality" of modern civilization.

Objecting to "the narrow empiricism of sense-data," he maintains that passion is not mere reaction but response: the experience of love, beauty, and awe has an ontological import. The mystic's vision and the scientist's law are both revelatory—each incomplete but neither false. But theology should be stripped down to "the stark simplicities of legitimate certitude," and the exclusive particularisms of the warring creeds replaced by a "reverence for reverence." Like Toynbee, Hocking believes that "the day of private and local religions is over," but he eschews a vague and devitalized eclecticism. Accordingly he devotes considerable space to the discussion of Christianity, which he regards as the most mature of the world's religions.

Much of what he asserts is subject neither to proof nor to rebuttal. Such is his frequent reference to the mystic's "awareness that the source of things is a Thou." The Being thus mystically intuited is said to be commensurate with the cosmos, yet to have an "intimate and individual concern" for each one of us, "as a parent for child." But the more cautious mystic and the nonmystical philosopher, mindful of the delusive nature of many visions, will continue to be wary. Perhaps "legitimate certitude" is considerably more stark than Hocking supposes.

His emphasis upon the necessity of a morale of consent is sound, and his analysis of our "split-mentality" is illuminating. But I think that he exaggerates the causal efficacy of ideas as distinguished from social processes and institutions. If his diagnosis is one-sided, so is his prescription. The state of the world demands action in many fields at once: economic, political, scientific, artistic, and moral; both inner and outer, ideological and institutional. Hocking puts all the emphasis upon religious and philosophical reconstruction.

MELVIN RADER

University of Washington

A SOURCE BOOK IN INDIAN PHILOSOPHY. Edited by SARVEPALLI RADHAKRISHNAN and CHARLES A. MOORE. Princeton, Princeton University Press, 1957. Pp. xxix, 684. $5.00.

This is the book which students of Indian philosophy have been awaiting for several years. And it was worth waiting for. In terms of the purpose which it is intended to meet it is an excellent achievement, and the publishers have brought it out in attractive form. One hopes that the remaining volumes which are expected to follow it—dealing in the same way with Chinese philosophy, Buddhist philosophy, and Japanese philosophy—will appear soon and will measure up to the high standard set by the present book.

I believe this is the first time I have written a review when no negative criticism of any kind seemed to me warranted. One could of course quarrel with the particular selections chosen for this volume; one could say that this or that introduction is too full or not quite sufficient; one who is acquainted with Sanskrit could no doubt suggest alternative renderings of some passages that would seem to him to bring out their meaning more adequately. But except for this last mentioned possibility (where I am incompetent to judge) a critic would need to justify another way of using the space, and such justification would in every case be very difficult. The most obvious question that might be raised is whether the selections from Buddhism ought to be omitted, in view of the fact that a volume on Buddhist philosophy is to be included in the series. But such a volume may never succeed in appearing, and Indian thinkers have always regarded Buddhism as a significant part of their philosophic heritage; indeed, its influence on developments since Buddha's time is very marked. So what other decision could the editors make?

Except where otherwise noted, the introductions were written by

Dr. Radhakrishnan and the detailed work of preparing the selections was carried out by Dr. Moore. In a few cases, sources were translated specifically for this book; in all others a judicious choice has been made among translations already available. Some revised renderings of certain passages have been made, and transliteration of Sanskrit terms has been standardized. The general introduction is the best brief survey of Indian philosophy that I have come across.

The volume opens with a few selections from the *Rig Veda*, which are followed by passages from the thirteen most influential Upanishads; two of the shortest are given in full. Then, from the epic period, we find the *Bhagavad-Gita* (entire), and selections from the rest of the *Mahabharata*, from the Laws of Manu, and from Kautilya's *Arthasastra*—a treatise on politics, economics, and diplomacy. After this section come selections from the so-called "heterodox" systems of philosophy—the Carvaka materialists, Jainism, and Buddhism. Two hundred and twenty-five pages are devoted to the "orthodox" systems, which accept the authority of the Vedas and are regarded in Indian thought, generally speaking, as complementary rather than opposing philosophies. These comprise the Nyaya theory of logic, the Vaisesika metaphysical atomism, the Samkhya dualism, the Yoga system of physical and mental discipline, the Purva Mimamsa philosophy of social ethics, and the Vedanta synthesis of ideas drawn from all these sources. The last section, on contemporary Indian thought, contains brief selections from Sri Aurobindo and from Dr. Radhakrishnan himself. A careful bibliography and index are added.

No one interested in Indian thought who does not expect to master the original materials can dispense with this book.

E. A. Burtt

Cornell University

SYMBOL AND REALITY. Studies in the philosophy of Ernst Cassirer. By Carl H. Hamburg. The Hague, Martinus Nijhoff, 1956. Pp. viii, 172. 12.40 guilders.

Professor Hamburg's book is not an organized exposition of Cassirer's philosophy; it is a group of essays, some of them reworkings of material published earlier, and it falls roughly into three parts. The first (Chapters I, II) tries to sketch a view of past philosophy as it must look from Cassirer's position; the second (Chapters III, IV, V) expounds and defends some aspects of Cassirer's philosophy of language; the third (Chapters VI, VII) tries to show how Cassirer's

philosophy differs from that of "positivistic semioticians" and concludes with a criticism of positivism.

The book has no unity. The last essay has nothing to do with Cassirer. It attacks the "verification principle" and illegitimately draws from its argument the conclusion that metaphysics is justified. The first part offers the general thesis that every philosophy does or must view from its own position all other philosophy, and maintains in particular that from Cassirer's point of view all previous philosophies are more or less mistaken attempts "to get beyond the pictorial and discursive symbolism of language, art, and the sciences to a direct apprehension of the 'Real' as such." Hamburg then tries to document this particular thesis by some sketchy accounts of Heraclitus, Plato, Aristotle, Descartes, Leibniz, Locke, Berkeley, and Kant. Nothing in them adds to an exposition of Cassirer. Moreover, Hamburg fails to see that the only evidence for the argument must come from Cassirer. Thus he mistakenly examines these philosophers to provide support for his thesis. What he does provide is a set of vague statements not worth contesting. There is not much to say, for instance, to the claim that the dialogues of Plato are illustrations of the method of turning ordinary language into an ideal language except that it is groundless.

Only Chapters III, IV, and V are worth attention. These deal with Cassirer's general doctrine that the attempt to distinguish between language and the world is mistaken. A language is a system of categorial devices which give "form" to the world. "To ask what reality is apart from these forms of language becomes irrelevant . . . every form of existence has its source in some peculiar way of seeing, some intellectual formulation and intuition of meaning." Cassirer holds that there are several "languages" or "perspectives," each with its own "reality." These are, roughly, the world of common sense, the world of myth and art, and the world of science.

How does one go about establishing the claim that language both shapes the world and symbolizes it? Hamburg cites from Cassirer's writings purported evidence drawn from psychology, physics, and linguistics. Yet Hamburg is also aware, though he blurs its significance, that it does not make sense to talk of offering empirical evidence for such a view. He points out that for Cassirer the world we investigate is one already shaped by a language. If we cannot "see" the world except through language, there is no getting outside the limits of language to verify the fact.

The mistake of trying to give empirical evidence for the thesis is compounded by Cassirer's maintaining that one's needs shape the

categories of one's language, that one can deliberately change one's perspective and "see" the world as one's needs dictate. For this puts the individual and his needs outside the language (Hamburg's disclaimer that the needs and languages mutually influence one another misses the point), and raises at the same time the question of what these languages or perspectives are perspectives of. Such a philosophy needs to explain how it can consistently talk of viewing the "same" object from different perspectives and yet deny the existence of anything other than language. An adequate account of Cassirer's philosophy of language needs to show how it can dispense with or account for the given, that is, something which is symbolized but which is not itself a symbol. Hamburg's book fails to do so.

PAUL WELSH

Duke University

AN INTRODUCTION TO THE PHILOSOPHY OF EDUCATION. By D. J. O'CONNOR. New York, Philosophical Library, 1957. Pp. vii, 148. $3.75.

Professor O'Connor writes this book in order to provide "a simple introduction to philosophical thinking" for students of education, and an elementary examination of "the most obvious points of contact between philosophy and educational theory" (p. v). The philosophical thinking to which he introduces the student is in his own words "contemporary 'philosophical analysis' "; consequently, the obvious points of contact are those at which analytical philosophy touches theory of education. These points of contact "comprise what might properly be called 'the philosophy of education' " (p. 138), and his book presents an analytical philosophy of that subject. As such, it is something close to unique. Its novelty is enhanced by a plain, simple, transparent style; and anyone interested in education or in philosophy of education will find it well worth the reading.

Philosophical thinking, in O'Connor's view, is "an activity of criticism or clarification" (p. 4), and his criticism leads to a criterion of the meaningfulness of statements, to a definition of "theory," and to certain views concerning morality and religion. The criterion of meaningfulness is expressed in the question, "What kind of evidence would be relevant to establishing the truth or falsehood of a statement?" If we can give no answer, the statement is meaningless. By use of this criterion, he charges theology, metaphysics, and other parts of traditional philosophy with unintelligibility (pp. 30-35).

O'Connor defines "theory" by using physics and astronomy as paradigm cases, stating that the term refers to an hypothesis (or logically interconnected set of hypotheses) which has been confirmed by observation (p. 76). The word, he says, has other meanings; but it is this one which is most important for his purposes.

O'Connor considers (chapter 3) two kinds of statements in morality: those which concern means and those which concern ends. The former state that certain actions are efficient in the production of certain results, and are for this reason simply one class of statements of fact. Statements of ends, on the other hand, cannot be reduced to statements of fact, and are properly understood only as expressing attitudes, persuading others to their adoption, and claiming universal acceptability for that upon which the attitude is directed. They are based upon fact, but their justification, neither deductive nor inductive, is not at present understood. In particular, they cannot be justified by statements of religion. For these contain no value concepts and this rules them out as premises for arguments concluding in value statements (pp. 127-128); and in any case, the value of ends, since it is independent of God's will, cannot be justified by reference to it (pp. 132-138).

The points of contact between philosophy and theory of education (that is, philosophy of education) are expressed in the questions: "Is theory of education theory properly speaking?" and "What is the proper bearing of religion on theory of education?" O'Connor says that there are other philosophical questions concerning the subject, but treats only these at length.

The answer to the first question is a trifle unclear because O'Connor offers different descriptions of theory of education (pp. 5-6, 104-110). The phrase applies to many bodies of thought which are very different one from another, and it may be the difficulty of describing in a general way such varied systems which accounts for the divergent descriptions of what are more precisely "theories" rather than "theory" of education. But if we put his descriptions together, a procedure which may be unjust to the author, it appears that each theory of education exhibits a certain form. Each consists of three bodies of statements: some set forth "techniques of imparting knowledge, skills, and attitudes" (p. 5); others would explain these techniques; and still others state the values in the light of which the techniques are employed. The first are based upon millennia of experience and are susceptible of verification. The explanatory statements are of two sorts: those drawn from the sciences, especially psychology (e.g., laws of learning);

and those drawn from metaphysics (e.g., the statement that all men have souls in Plato's theory). Taken together, statements of these kinds in each theory would explain its statements of technique by treating the latter as laws of human behavior under conditions of teaching, and by providing for them premises of a more general kind from which they can be deduced, as laws in the natural sciences are explained by deduction from more comprehensive laws (pp. 85-87). Statements of the third kind, as we have seen, are expressive, persuasive, and clamatory.

Systems of statements of this kind are not theories in O'Connor's view. A theory is in fact verified as true or probable by experimental observation. Statements of technique, although susceptible of it, have not been subjected to enough experiment to be regarded as confirmed. Metaphysical statements, included among the explanatory, are incapable of empirical test; and scientific statements found there, for example those concerning conditions for learning, have not yet received sufficient confirmation to command universal agreement. Statements of this explanatory sort, moreover, cannot in fact explain statements of technique since the latter can be deduced from incompatible statements of this second sort in different theories. Statements of value, for example those concerning training for democracy, can have no empirical confirmation, however useful they may be in activating the information embodied in statements of technique. No part of a theory of education is, properly speaking, a theory; and no part is logically connected with another in the way in which one part of a genuine theory, a natural science, is connected with another which it explains. Consequently, the whole is not an explanatory theory; and that term as it occurs in the phrase "theory of education" is no more than "a courtesy title" (p. 110).

The second question in philosophy of education treated by O'Connor is "What is the proper bearing of religion upon theory of education?" His answer is that there is none. We have seen that religious doctrines cannot justify value statements; consequently, they cannot justify the statements of value in theory of education. The assertion that its bearing on education consists in the fact that religious belief causes better conduct is susceptible of empirical test although such a test would be very difficult to administer; and a very little available evidence suggests that church affiliation fosters crime if not immorality.

The reader may conclude from O'Connor's discussion that philosophy of education is the continuing activity of critically inspecting theories (courteously so called) concerning the practice of education;

and that at present these theories provide a very poor instrument for understanding educational processes directed toward objectives whose justification, certainly not religious, is also not understood.

Certain features of O'Connor's book may puzzle the reader. First, a criterion of the meaningfulness of statements seems to be advanced both as the sole criterion and as one among many possible alternative criteria. Theology and metaphysics are condemned as meaningless because we cannot know what kind of evidence would be relevant to them; but O'Connor also argues that language has many employments other than the informative (p. 36) for understanding which criteria can presumably be offered. Moreover, the question, "How do we recognize evidence relevant to a statement before we understand its meaning, i.e., in logical independence of its meaningfulness?" — a question which defenders of this criterion must answer—is nowhere discussed.

Second, the word "theory" is used in such a way that no hypothesis can be a theory unless it is in fact known to be true or probable. This makes it impossible to describe the activity of education as based on any theory except one about whose truth-value no sensible question can be raised—neither a theory which is false nor even one which is as yet unverified. This seems an unduly rigorous limitation upon the reference of the term. Besides, it seems out of keeping with one important tendency in analytical philosophizing, that of admitting anything to be a so-and-so which bears the title. According to this tendency, there can be no such things as "courtesy titles" although things of different sorts may receive the same appellations; and theories of education are, in this vein of thought, quite as much theories as physics, however much they may differ from that science.

Third, the exposition of analytical philosophy suffers from the absence of any detailed statement of the positions of traditional philosophy against which, as O'Connor expounds and engages in it, analysis makes war. And the force of his analysis of theory of education, similarly, suffers from an absence of any detailed statement of particular instances of that theory.

Some of these difficulties will be remedied by the knowledge which students of education and those interested in its philosophy bring to O'Connor's book, and none need puzzle the reader who keeps them clearly in mind. The book will succeed in introducing such readers to analytical philosophy; and they will find in it a serious presentation of an important point of view. For students of education, the book is a helpful counterweight to literature in the theory of that subject with

which, in English speaking countries, they perforce familiarize, if they do not burden, themselves. For those interested in philosophy of education, the book is a forceful measure toward its revitalization, and a hopeful sign that it is shaking itself out of its doldrums.

KINGSLEY PRICE

The Johns Hopkins University

PHILOSOPHY, POLITICS AND SOCIETY. A Collection Edited by PETER LASLETT. Oxford, Basil Blackwell; New York, The Macmillan Company, 1956. Pp. 184. $3.00.

One of the announced objects of this collection of recent English essays is to illustrate the effects on philosophical writing about politics of the ambitions and activities of social science, on the one hand, and the new preoccupation of philosophers with questions of language and meaning, on the other. This object might have been pursued and gained without anything much being done toward achieving the quite different object of indicating just what range of topics, old and new, deserve the attentions of a reformed political philosophy; Mr. Laslett, however, in his capacity as editor, signifies that he has had this object in mind, too.

How far does the collection go toward achieving the second object? I think not very far. It is true that none of the other essayists is quite so unmindful of the larger world of science and learning as Renford Bambrough shows himself when, more insular than insularity alone could account for, he announces, "Although only Professor Gilbert Ryle seems to have acted on it, there is a growing belief that the interpretation of Plato and the elucidation of philosophical questions and doctrines can profitably be combined" (p. 99). (The reference is to Ryle's study of the *Parmenides*.) Nevertheless, the collection as a whole ignores any chances of reorienting political philosophy according to the findings of contemporary social science, instead of profiting from those chances; and even on the standard repertory topics to which the collection is for the most part confined, a great deal less than the full imaginative resources of analytical philosophy is employed.

Laslett's own contribution, "The Face to Face Society," is the only essay that seems at all sensitive to the disclosures which social science has made regarding the actual setting of political choice. His notion that political philosophers have yet to understand the relationships between the "directive face to face group," on which the task of

government in large nations must, according to Laslett, devolve, and the "territorial society" which it directs by manipulating explicit symbols (in default of the intuitive responses that suffice in a face to face society) brings together several themes of recent social research. But it is difficult to judge whether Laslett is aware of the affinities between these themes and his speculations. His only reference to actual work in social science is a gratuitously slighting one to psephology. He does not seem to know of major accomplishments in the study of voting behavior like *Voting*, the Elmira study by Berelson, Lazarsfeld, and McPhee; of works on the sociology of political influence like David Truman's *The Governmental Process;* or even of the flourishing branch of sociology that is studying the behavior of small groups. So his suggestions, happy ones when he does not garble them by bad argument, seem to be only accidentally related to social science.

Considered simply as a performance in philosophical analysis, A. M. Quinton's essay on punishment is easily the best thing in the collection. Quinton does offer, at the outset, an argument which purports to identify the retributive theory with the theory he happens to want to discuss, and this is both unnecessary and unconvincing; but he proceeds thereafter with unmarred efficiency. He ingeniously demonstrates that we cannot logically claim to punish people for things that they have not done; and argues from this fact that it is a misunderstanding of utilitarian recommendations to suppose that since they call for punishment to achieve certain effects, they prescribe punishing innocent people on occasion.

Second honors in the collection may be accorded "The Theory of Sovereignty Restated" by W. J. Rees. Rees catalogues the received meanings of "sovereign" in an exceptionally clear way and then, with the simple machinery of the disjunction between logical and causal necessity, disposes in good time and order of the several questions to which his analysis has reduced the traditional problem of whether a state must have a sovereign. He then proposes that some of the received meanings be discarded; and shows how legal sovereignty and coercive sovereignty may be kept distinct and yet each may be sensibly related to the other by definition. Unfortunately, Rees insists on complicating his definition with a third element, the useless and distracting idea of an "influential sovereign," since, for some odd reason, he seems to think that this idea must be retained if we are to be able to discuss influences on either a legal or a coercive sovereign.

Several of the other essays, though useful and therapeutic so far as they go, are oddly wanting in one of the particular virtues of contempo-

rary British philosophy, namely, imaginative patience in conceiving and explicating the various subtle tasks that language is used to perform. Thus Glanville Williams, in an otherwise acute and successful article on "The Controversy Concerning the Word 'Law'," quite fails to see that larger issues than mere "convenience" may hang upon a definition. If "law" is defined (with Austin) to exclude certain phenomena analogous to municipal law, then a descriptive theory of law developed with the definition may be less intelligible than it could have been, either as a matter of the psychology of students of jurisprudence, or, logically, by requiring an unreasonably complicated axiomatic system for describing the same larger range of facts. The late Margaret Macdonald, in an essay on "Natural Rights," struggles hard to undo the effect of her own less than subtle categories, but does not succeed in stifling the suggestion, which they engender, that it was simply cheating for the natural rights doctrine to disguise expressions of value with a claim to rationally ascertained facts. And Mr. Bambrough, though he is efficient enough in making some standard moves against Plato's conception of political wisdom, does not anticipate the tenacious plausibility with which the conception could be reformulated.

Progress in political philosophy will require subtler and more elaborate use of analytical techniques, as well as some attempt to perceive how the problems set for analysis have shifted in importance as a result of the activity of social scientists. It will also require, I think, an infusion of confidence. Professor M. J. Oakeshott, overdoing modesty, declares at the end of his inaugural lecture, which is reprinted here, that "political philosophy cannot be expected to increase our ability to be successful in political activity. It will not help us to distinguish between good and bad political projects; it has no power to guide or to direct us in the enterprise of pursuing the intimations of our tradition" (p. 20). And T. D. Weldon's disclaimer is almost as extravagant: "Philosophers are to make no claim to improve either the theories or the methods of those who are engaged on scientific or political activities" (p. 22). But isn't it an improvement to clarify the language of politics and the discourse of political science, and cannot philosophers contribute to this improvement without straying from their special tasks of analysis?

W. B. Gallie, in an essay on "Liberal Morality and Socialist Morality," suggests that if philosophers spent some effort "on current issues relating closely to political morality, and ... aimed at unmasking fundamental differences rather than at reaching superficial agreement," this "*might* do something towards illuminating our contemporary

moral world: *might* help to dispel that thin but bewildering fog which seems to surround so much current ethical discussion and to make its terms and distinctions so unearthly and unreal" (p. 133). Thus it may be that excursions into political philosophy would not only help improve political science and political behavior; it might also advance the discussion of topics peculiarly central to philosophy. But only Gallie, of this whole set of writers, expresses so much constructive optimism.

DAVID BRAYBROOKE

Yale University

THE PROBLEM OF UNIVERSALS. A Symposium. By I. M. BOCHENSKI, ALONZO CHURCH, NELSON GOODMAN. Notre Dame, Ind., University of Notre Dame Press, 1956. Pp. 54. $.95.

This brochure consists of three papers which were read at the Aquinas Symposium sponsored by the Department of Philosophy of the University of Notre Dame on March 9 and 10, 1956. The first paper is "Propositions and Sentences "by Professor Church; the second, by Professor Goodman, is "A World of Individuals"; and the third, by Professor Bochenski, bears the title "The Problem of Universals." In an explanatory introduction these papers are described as "literally" making up a symposium and as being "papers that converge on the Problem of the Universals from three different philosophic positions." Apparently they are a selection from the papers given at the two-day meeting and were not prepared with the intention of making contributions to a connected discussion of a specified problem relating to universals, one which normally would attempt to canvass differences of opinion between the participants. The convergence which the writer of the explanatory introduction finds in the three essays is invisible to this reviewer. It is possible, for example, both to be a nominalist in Goodman's sense of "nominalism" and to reject nominalism in Church's sense, and it is possible consistently to reject nominalism in Goodman's sense while being a nominalist in Church's sense. This can hardly be taken to indicate convergence on *the* problem of universals.

Church in his essay gives a brief account of the historical background, from the Stoics to the present, of two current senses of the term "proposition" in philosophy: (1) the traditional sense of declarative sentence together with its meaning, and (2) the abstract sense of "content of meaning" of a declarative sentence (p. 5). The first sense implies

that there are as many propositions as there are translations of a given declarative sentence into other languages, whereas the second sense implies that there is only one proposition which is expressed by the sentence and its various translations. Church thinks that many logicians find the traditional sense awkward or unsatisfactory and that there is a definite need for the abstract notion, at least for some purposes. It is not easy to see why any logician should find the first sense awkward; for if they do not wish to use the word "proposition" in that sense and wish to use it in the abstract sense they are at liberty to do so, as that sense exists and is available. A reasonable construction to be placed on Church's plea for the abstract sense is that he wishes it to be made the *exclusive* sense of the word "proposition." The puzzling thing is why he did not proceed directly and without ado to his point, and also why he did not abandon the word to its two present senses and introduce a new term which would have only the abstract sense. What is the aura about the word "proposition" which should make it a battleground for exclusive possession? Church goes on to give Leibniz's considerations in *Dialogus de connexione inter res et verba* for the view that whatever is the "subject of truth" is *something* which is neither a thought nor a thing nor yet an actual sentence, but is a *possible* thought or a *possible* proposition, or perhaps even a possible sentence (p. 9). The expressions "possible thought" and "possible proposition" are construed to mean proposition in the abstract sense; and Church contends that interpreting Leibniz's "possible thought" to mean possible sentence does not play into the nominalist position, as possible sentences are "not particulars but universals" (p. 8). This supplements his contention, developed elsewhere, that there is something other than a sentence which is the subject of an assertion and of a belief.

Professor Goodman attempts, in "A World of Individuals," to make clear his position, which he identifies as nominalistic, and to defend it against a number of criticisms. His version of nominalism is that it consists in the refusal to countenance classes: ". . . the nominalist rejects classes as incomprehensible, but may take anything whatever as an individual," where this rejection "does not involve excluding abstract entities" (p. 17). It is clear, then, that Goodman's nominalism is compatible with Church's Platonism. For it is possible consistently to hold that there are "contents of meaning" which are the subjects of truth and belief, actual or possible, and at the same time to reject classes as incomprehensible; and it is possible to reject Goodman's nominalism and accept classes as perfectly comprehensible while

disavowing abstract entities, in particular propositions in the sense of G. E. Moore and Church. However unlikely it may be that Church would accept Goodman's view about the incomprehensibility of classes, the specific views they advance in their papers are not incompatible despite terminological appearances. But what is the reason for rejecting classes as incomprehensible? It is that classes go against Goodman's maxim that two different entities cannot be "made up" of the same entities (p. 18). Given k initial elements or individuals, Goodman allows that there exist $2^k - 1$ "wholes" or "individual sums" of the elements, where a condition for anything being a whole is that it consist of one or more elements and only of elements (p. 19). The use of "whole" is thus very different from the use of "class": there cannot be a null whole nor a whole of wholes, as there can be a null class and a class of classes, a class of classes of classes, "and so on ad infinitum, climbing up through an explosively expanding universe towards a prodigiously teeming Platonic heaven" (p. 19). Goodman thinks that his position is "virtually unassailable" (p. 31), and it must be admitted that it is unassailable. For his arguments against classes do no more than bring to our attention features of the logical use of the word "class" which have for some reason become distasteful to him; and he cannot of course be prevented from rejecting this use of the word. But the position that classes exist and are perfectly comprehensible is equally unassailable; logicians who accept it are not unaware of these features. Instead of inducing semantic agoraphobia in them, the fact that classes generate an "explosively expanding universe" is accepted as a desirable feature.

Bochenski in his paper, from which the symposium under review takes its name, states "provisionally" what he considers to be *the* problem of universals, namely, "Are there universals?" He declares at the outset that he takes the position that "every extra-mental thing in the universe is singular, not universal" (p. 35). And on the next page he writes, "When we know things, we form in our minds certain images, concepts, etc. It must be stressed that those subjective entities are *real*, consequently individual, being parts of an individual mind." It would seem that with these two declarations (and, indeed, with the second alone: "*real*, consequently individual") Professor Bochenski gives us his answer to the question "Are there universals?" His answer, by obvious and direct implication, is that there are no universals. Nevertheless, he states that he will not attempt to arrive at the solution of the problem. According to his own account he confines himself to restating some of its aspects "in the frame of recent logical

semiotical techniques, and in face of some new ontological doctrines" (p. 35), a restatement which requires the investigation of five "levels or realms of reality" (p. 36).

This reviewer must confess to being puzzled by a number of things in the three papers. To touch on only one: if one question as to whether there are universals is the question as to whether declarative sentences have meanings, "contents of meaning," it is not easy to see how there could be any disagreement over it. Nor is it easy to see why one philosopher should think it necessary to produce *arguments* for the existence of universals or "contents of meaning," and why another should think that special "logical semiotical techniques" are needed to help determine this.

MORRIS LAZEROWITZ

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TERMINALKAUSALITÄT ALS DIE GRUNDLAGE EINES UNITARISCHEN NATURBEGRIFFS. Eine naturphilosophische Untersuchung. Erster Teil. Terminalkausalität in der Atomdynamik. By EINO KAILA. Helsinki, Acta Philsophica Fennica, Fasc. X, 1956. Pp. 122.

This is the first of two books designed to establish four propositions: (1) *Initialkausalität* (IK), the schema of classical physics, cannot even in principle accomodate the "holistic" and "finalistic" features of biology and psychology. (2) Atomic physics (the quantum *theory*) does not exemplify IK either. (3) It does exemplify the schema of *Terminalkausalität* (TK). (4) So also do biology and psychology. In this volume the author first expounds the notion of TK, then argues (3). In the sequel he hopes to argue (1) and (4). (1) is unwarranted, to say the least. (2) is true for the semiclassical model, false for quantum *mechanics*. Whether (3) and (4) make sense depends on whether TK does.

Let S be a physical system, B its (geometrical) boundary. Let S^o and S^t stand for the values of a *complete* set of relevant variables of S at *moments* o and t respectively; B^t_o for the value courses during the *interval* (o, t) of those among these variables that are *located in* B. B^t_o is known as a boundary condition. A process law P permits the computation of S^t from S^o and B^t_o. This is the schema IK. Sometimes one can, by means of P, infer from B^t_o *alone* not S^t, to be sure, but certain properties of and relations among its several constituents. Sometimes, again, one may know laws of this kind before P has been discovered.

(Thus there may be no P to be discovered.) Bits of knowledge (laws) of this kind are said to exemplify TK. That is all we are explicitly told about the crucial notion. It does not suffice to establish a "schema" to be contrasted with IK. Schemawise, all one can say about such laws is that they are less than process knowledge in the sense that, *if* there is a P in the area, they can, *after* it has been discovered, be deduced from it. The notion of TK thus not only remains elusive. It is, I fear, irremediably confused. To support this judgment, I mention, first, that very oddly the relativity axiom (constancy of the speed of light) is held to exemplify TK; and I mention second that the author is quite patently under the spell of accidental verbal connections (*terminus* = boundary).

A notion inherently confused cannot be used to clarify anything. It follows that, if I am right about TK, the third of the four propositions above is moot. This is borne out by what is said, in the last two-thirds of the book, about the quantum *theory*. This theory talks about three kinds of things: (*I*) observables, (*II*) the entities of the semiclassical model, and (*III*) those of quantum *mechanics*. Coordination proceeds from *I* through *II* to *III*, interpretation in the other direction. For example, the system of *III* corresponding to a n-body system of *II* is a set of "wave" functions (ψ's) spread throughout a $3n$-dimensional phase space. The distinction between the three levels is the very key to the logical structure of the theory. After a fashion the author knows this. I say "after a fashion" because he believes nevertheless that the many-dimensional systems of the phase spaces can somehow be "interpreted" as waves (*Schwingungen*) in physical space. This is in fact his central idea. One need not know much physics in order to know that it is utterly absurd. So we must search for the motive behind this almost incredible blunder. The author obliges. If he were right, the so-called quantum conditions could be thought of as the boundary conditions of the *Schwingung*, in analogy to, say, a classical fluid. This is where TK comes in.

The author insists that he uses "holistic" and "finalistic" descriptively, that is, not as the philosophical holists do. In a sense he does; at least he tries. But I do not think that he always succeeds. Also, he disparages explicit definitions and extols the so-called implicit ones. This bias, I am prepared to argue, is an infallible symptom of philosophical holism. It appears most patently in an inconclusive criticism of Mach's analysis of the classical mass notion.

Kaila seems to be very well read in science and in what scientists write about it. Perhaps that is part of the trouble. At any rate, he cites

many details I never heard of before (and hope never to hear of again). In philosophy his record is long and distinguished. So I shall say no more than that I wish this book had never been written.

GUSTAV BERGMANN

State University of Iowa

DESCARTES'S RULES FOR THE DIRECTION OF THE MIND. By the late HAROLD H. JOACHIM. Reconstructed from notes taken by his pupils. Edited by ERROL E. HARRIS. Foreword by SIR DAVID ROSS. London, George Allen & Unwin, Ltd.; New York, The Macmillan Company, 1957. Pp. 124. $2.50.

This book, based upon the lecture notes of two of Joachim's former pupils, Professors Errol Harris and John Austin, contains the substance of the lectures on the *Rules* given in the early thirties. In the first chapter, "The Power of Knowing," Joachim goes directly to the heart of Descartes' epistemology by expounding and criticizing the notions of intuition, deduction, simple nature, and enumeration. In the second chapter, "The Cartesian Method," he discusses the distinction between absolute and relative natures, the analysis of complexes into their constituent simples and the complementary synthesis, the doctrine of the universal mathematics, and the role of imagination. In the third chapter, "Philosophical Analysis of the Concrete," he offers his own "concrete organic unities" in opposition to the Cartesian complex natures which he regards, not unexpectedly, as aggregates of atomistic, isolatable simples. While the last chapter will be of interest to those who are concerned with Joachim's own position, the principal value of the book resides in the first two chapters which constitute an interesting critical commentary on the main doctrines of the *Rules*.

If the reader should be inclined to ignore this study on the grounds that it is colored by a philosophic outlook now outmoded and that it has in all likelihood been surpassed by more recent Cartesian scholarship, he will be doing it a great injustice. To be sure, Joachim's idealism is reflected occasionally in analyses which are misleading, if not mistaken. For instance, his view that the simplest item of knowledge is the proposition and the related doctrine that the simplest act of the knowing mind involves both synthesis and analysis lead him to say that the Cartesian simple natures are really propositions involving a relation between two terms and that Descartes should have held that the simplest act of mind involves at one and the same time both

deductio and *intuitus*. His idealism is also reflected, however, by the topics he chooses for fuller discussion and by the emphases he makes, and it is just these things that make the lectures stand out after twenty years. He is led to concentrate on the crucial doctrines in the Cartesian epistemology and to explore persistently ambiguities and inconsistencies which might be considered more casually, if at all, by a non-idealistic reader. For instance, his preoccupation with the relationship between the whole and its parts draws his attention to the Cartesian concept of simple natures and leads him to consider, among other things, how Descartes could, in the light of his own discussions of abstraction and "distinction for the understanding," speak of complexes being constituted of distinguishable simples at all. If the contemporary reader should feel that Joachim speaks to him from another tradition, he becomes aware that that tradition has one of its roots in Descartes and that it provides a viewpoint from which some of the Cartesian difficulties stand out in bold relief.

Some of Joachim's arguments would have been polished up and others documented more fully had he himself prepared his manuscript for the press, but the book nevertheless exhibits the scholarship displayed in his other works. The reader may not agree with all the analyses, but he will be challenged to turn to Descartes and accompany Joachim through the appropriate Rules. This little book should be read by all students of Descartes.

LEONARD G. MILLER

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THE MORAL BASIS OF BURKE'S POLITICAL THOUGHT. By CHARLES PARKIN. Cambridge University Press, New York, 1956. Pp. viii, 145. $2.50.

Any book on Burke these days is going to be of interest—such is the current vogue not only of formal Conservatism but also of traditionalism as simply a general principle for the determination of social values. And Mr. Parkin's essay has the virtue of asking about Burke's philosophy the central question: how can we know which traditions are to be conserved as truly and prescriptively traditional? For Burke was never *merely* historical. Somewhere there was a moral basis to his thought, a basis by which he determined to urge on some history and to oppose the rest. But it cannot be said that Parkin's identification of that moral basis is particularly illuminating.

He has two major suggestions on this score. The first is that Burke's

"thought reaches back for its basis to a political theory of great antiquity and generality, the theory of the social contract." This is a challenging idea and also a rather silly one. There is no question that Burke made frequent use of the language of contract as did all philosophers of his era. But to say that this language—or that of Natural Rights—is the basis of his political thought does violence both to the categories by which we have come to classify thinkers and to the notion of contract itself. Burke's thought is not egalitarian or individualistic. Nor is the state for him an artificial convenience. Parkin admits all this, grants Burke's huge debt to Aristotle's politics as well as to Aristotle's ethics. But he says that Burke remained true to the contract tradition by showing himself dependent on the objective existence of a higher law. Yet is there not a similar, even more absolute, dependence in Thomas? To say that Burke could not have accepted the contract theory in any other way than he did—by drastically reinterpreting it beyond any recognition by Locke or Paine—does not prove more than that Burke was both very wise and very practical.

Parkin's other major suggestion for what lay at the heart of Burke's scattered thought is more orthodox. It is that "The source of moral truth, for Burke, is an eternal moral order which is immanent in the historical process." Even though the perception that at bottom Burke is a deeply religious man is one which should be obvious to any thoughtful reader of his writings, it is nevertheless a major perception and one which badly needs analysis. What was Burke's Christianity? Was it more Augustinian than Thomistic? Was there any of Luther in it? Modern Conservatives should be told the answers to these questions about their mentor. Parkin does not tell them. The major portion of his book is a flat, though thoroughly competent, review of Burke's declared philosophy. There is little analysis. The question Parkin set out to answer still stands: what made up that central core of settled belief lying behind all the variety of Burke's occasional writings?

H. Mark Roelofs

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NOTES

The Committee on Information Service of the American Philosophical Association is receiving numerous requests for the names of retired teachers of philosophy willing to take temporary appointments. Any qualified person, man or woman, who is interested in such an appointment is urged to register with the Committee. Communicate with the Chairman, Professor Paul C. Hayner, Washington and Lee University, Lexington, Virginia.

At the annual meeting of the Southwestern Philosophical Conference the name of the organization was changed to the Southwestern Philosophical Society. The officers for 1958 are: President, Professor Robert Rien'l, Louisiana State University; Vice President, Professor J. Clayton Fearer, University of Oklahoma; Secretary-Treasurer, Professor Ernest W. Dewey, Oklahoma State University.

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